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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE GEOLOGICAL HISTORY OF THE ANTILLEAN REGION[1]

IN selecting a subject for the address of the retiring vice-president of Section E, I have taken the most interesting but least known portion of one in which I have been very much interested during the past twenty years and more, namely, the paleogeography of North America. The portion dealt with on this occasion is the known geological development of the greater Antillean region, that is, the seas and lands bounded by the perimeters of the Gulf of Mexico and the Caribbean mediterranean.

The deciphering of the geological development of this greater Antillean region began with the versatile and philosophical Alexander von Humboldt. In June, 1799, he and the botanist Bonpland set out for Central and South America to study their physical geography and tropical botany. Humboldt first studied Venezuela and the Orinoco country, and later traveled more than a year in Cuba. After visiting western South America, he devoted the year 1803 to Central America. True to his training, Humboldt was an ardent Wernerian, and yet the leading student of volcanoes of his time. His most valuable results, however, are his geographic descriptions.

The geographic and geologic literature of the greater Antillean region is very voluminous, embracing the results of a host of workers, widely scattered in several languages. It is, indeed, altogether too extensive to be presented in brief form. The first important work on the stratigraphy and structure of the Greater Antilles is the report on the "Geology of Jamaica" by James G. Sawkins and his English associates, published in 1869. But the father of Antillean geology is undoubtedly Robert T. Hill, whose work in Panama, Jamaica, Cuba and the Lesser Antilles forms the broad foundation on which all subsequent work must be built. The stratigraphic succession, and especially the marine faunal correlation from place to place, have been worked out more recently by T. Wayland Vaughan and his associates. Voluminous additional paleontologic work is by Gil-

[1] Address of the vice-president of Section E—Geology, American Association for the Advancement of Science, New York, N. Y., December 28, 1928.

bert D. Harris and students trained by him. Our knowledge of Haiti we owe to W. P. Woodring and C. Wythe Cooke; that of Porto Rico, to Charles P. Berkey and his coworkers of the New York Academy of Sciences; while the intricate problem of the coral islands, as seen in the Lesser Antilles, is ably presented by W. M. Davis in two recent books.

The grander geological features. Before turning to the detailed geology of this part of the world, it will be well to take a rapid look at the more general geology of the lands bounding the Gulf of Mexico and the Caribbean mediterranean. The clearly dated geologic history of the Greater Antilles is more especially of Cretaceous and Cenozoic times, and only in western Cuba are there fossiliferous strata of Middle and Upper Jurassic ages. Beneath the latter, however, there is a deformed and metamorphosed basement, usually ascribed to the Paleozoic, which on the basis of Central American history should be of Permian and Carboniferous formations.

By general assent the Greater Antilles include Cuba, Jamaica, Haiti-Santo Domingo, Porto Rico and the Virgin Islands. Cuba is clearly the oldest of the Antillean islands, already in existence in Paleozoic time. The other islands begin with volcanics, most of which appear to be of Upper Cretaceous time, but those in Porto Rico are of Lower Cretaceous age; all seem to lie upon a Paleozoic foundation. To the east of the Virgins are other limestone-capped islands, most of which also belong to the Greater Antillean arc. Probably all of these northeastern Windward Islands are submarine volcanic growths of early Cenozoic time, which have risen into lands at different times, been beveled across, then subsided deeply and accumulated a thick cap of limestones. All the northern states bordering the Gulf of Mexico are also of Cretaceous and Cenozoic times, and nearly all of eastern Mexico as well.

Central America, however, has an ancient core of deformed Pre-Cambrian crystallines and metamorphics, which are well exposed in Honduras, southern Guatemala and Chiapas, and Oaxaca. This old protaxis disappears beneath the Mesozoic covering strata of southwestern Mexico. The Central American part of this protaxis trends east and west, and is slightly curved to the south. It is widely overlapped on the north flank by Upper Carboniferous, Permian, Cretaceous and Cenozoic formations, but on its southern side it is questionable if there ever were marine overlaps older than the Cenozoic. On the other hand, there was no Costa Rica and Panama apparently before late Cretaceous time.

The northern area of South America in Venezuela and Colombia is also very largely composed of Cretaceous and Cenozoic formations, and while there are older ones in the mountain cores of these republics, none appear to go back of the Middle Devonian. These fossiliferous Paleozoic strata, however, rest on an older foundation that apparently is of Proterozoic time. Much of the northern borderland is now foundered into the depths of the Caribbean Sea, and the island of Barbados is its most northeasterly outpost. Tobago and northern Trinidad are other portions of this foundered borderland, which has been called Paria by Guppy.

This much-generalized account of the broader geology of the Central American-Antillean lands seems to bring out the fact that in late Proterozoic time the Cordillera of western North America ended before attaining Oaxaca. Here another ancient protaxis appears with a northwest-southeast strike, but soon changes its course to one with a nearly east and west direction but convex to the south, and so continues through Honduras and apparently across the Caribbean Sea into Jamaica and possibly even beyond Haiti and Porto Rico. To the east of Haiti the old protaxis subsides more and more, and is covered with late Mesozoic and early Cenozoic volcanics.

Appearance of the Gulf of Mexico. Immediately to the north of this Central American-Antillean protaxis there lies a folded foreland that was much the widest across Cuba, and beyond it occurs what Suess has called the "Gulf of Mexico plate." This "plate" of nearly horizontal strata also includes the states bordering the Gulf of Mexico from Texas on the west to Florida on the east, and the latter state appears to have continued unbroken into the Bahama banks. During Paleozoic time this foreland-plate subsided very little, even though the waters of the Atlantic Ocean extended across it as shallow seas to enter the geosynclines of Mexico and Appalachis. Finally, in the Middle Jurassic, there began in the northern hemisphere a great oceanic transgression, and in keeping with it first Mexico and then the Gulf of Mexico began to subside, though most of the present form of the Gulf came with the late Cretaceous. Its greater depth, on the other hand, was developed especially during the Cretaceous and Eocene, with further deepening during Oligocene and Pliocene times. Even now the bottom of the Gulf of Mexico is not creased with folds or much disfigured by normal faulting, as is that of the Caribbean Sea. On all sides it is a gradually deepening basin going down to over 12,000 feet, except in the east where it is deformed by upfaulted Yucatan and the adjacent downfaulted Yucatan straits; both blocks are tilted to the west.

Caribbean mediterranean. To the south of the Central American-Antillean protaxis the structural condition of the Caribbean mediterranean is very different. Here the Antillean borderland fronting the Caribbean Sea was always narrow and much of it has sunk into the abyss. The Caribbean waterway is now an abyssal and overdeepened one, but it appears to have had at all times depths greater than 10,000 feet, and is regarded by Suess as the western end of his Tethyian mediterranean. It differs from the Gulf of Mexico not only in being a vastly older and far more complex basin situated between two continents, but also in being greater in area and depth. Furthermore, its bottom shows northeast-southwest trending folds and troughs; and in addition it consists of two basins, a greater western and more folded one, with depths down to 14,100 feet, ending south of Haiti and Porto Rico in a supplementary fault trough 17,100 feet deep, and a smaller but still deeper eastern and less folded trough, the Tanner Deep, with a maximum depth of over 17,000 feet. On the other hand, in Cenozoic time most of the Parian borderland of South America was also down-faulted to depths ranging to nearly 17,000 feet, and much of western Antillea was faulted into the abyss during the Pliocene, changing the former structural synclinal valley here into the long and narrow Bartlett Deep with present depths of over 19,200 feet. Farther north, other parts of Antillea were down-faulted into a less deep basin, the Antillean Sea, which goes down, however, to nearly 15,000 feet.

Costa Rica-Panama land-bridge. Now the question arises, when was the ancient Caribbean opening into the Pacific closed by the land-bridge of Panama and Costa Rica that now unites South and North America? The direct evidence of the oldest volcanics in Panama shows that they are unconformably overlain by late Eocene faunas (Claibornian), and accordingly this basement is usually assigned to the early Eocene. On the basis of the late Mesozoic faunas, however, the California ones are seen to be so different from those of the same age in Mexico and the Gulf States as to support the conclusion that the land-bridge was more or less completed during Lower and Upper Cretaceous time. Its formation appears to have been started by the late Jurassic crustal movements, which developed here a submarine ridge that was studded by volcanoes. Previous to the late Jurassic, however, the Caribbean is believed to have been widely open into the Pacific.

The early Cenozoic stratigraphy of southern Central America and that of California and the West Indies region are everywhere so much alike that it is plain that the Costa Rican portal was again more or less widely open as a shallow sea beginning with the Upper Eocene (Claibornian) and ceasing with the close of Lower Miocene time. Since then, elevation has here dominated over subsidence, and the Costa Rican-Panama portal has remained closed. However, another but short-lived marine portal came into existence, this time to the north of Central America across the Isthmus of Tehuantepec. It was certainly open during early Pliocene time, and probably also during the late Miocene, permitting the marine faunas of the Gulf of Mexico to spread west and thence north into southern California as far as Carrizo Creek, where Atlantic corals and mollusks are known.

Central American-Antillean land-bridge. Previous to the late Eocene and apparently back into Proterozoic time, it is certain that Central America connected widely across the Honduran-Nicaraguan banks with Jamaica and Haiti-Santo Domingo. It was then easy for the floras and faunas of both North and South America to spread into the Greater Antilles. Beginning probably as early as the Middle Eocene, the South American connection was again severed by the Panama-Costa Rica portal, a shallow sea that continued almost into Middle Miocene time. To the north of Nicaragua, however, there was during this time continuous land into North America. On the other hand, late in the Miocene and during early Pliocene time, the Tehuantepec portal came into existence, breaking this northern connection and stopping any South American migrants during this interval from getting into North America; but the waves of South American organisms could still spread to a limited extent into the Greater Antilles. With the Pliocene, the Antilles were completely separated into their present entities, while South and North America could freely interchange their life forms.

The Caribbean volcanic arc. In the east, the Caribbean Sea during Paleozoic and most of Mesozoic time appears to have been widely open to the Atlantic, but when the deeply submerged ridge came into existence on which the Lesser, volcanic Antilles originated, is not known. From this ridge there arose in the north, certainly as early as late Eocene time, many submarine volcanoes that made islands extending to Martinique in the south. This crescent of volcanoes is convex toward the Atlantic, and the completed series appears to have spread from north to south, with the Grenadines certainly the youngest of the series. There is, however, no direct geologic or submarine evidence to prove that the Caribbees at any time were a continuous land-bridge connecting South America with the Greater Antilles, as inferred by some zoogeographers. In the south these volcanoes are perched on the top of the subsided Parian border-

land, and in the north they are parasitic growths on the western side of the older volcanoes, now covered by limestones and making the eastern terminus of the Greater Antillean series.

Barbados. The geology of Barbados is well known through the work of English naturalists, and it is doubly interesting since the island is covered by 350 feet of unmistakable oceanic deposits. Barbados lies in a hinge, or very mobile area, between the continent of South America and the Caribbean abyss, and the oceanic deposits of the island include even Red Clay, giving good ground for holding that the subsidence may have been to a depth of 10,000 to 12,000 feet. The series begins with shallow-water deposits, followed by the oceanic series of Globigerina and radiolarian oozes, and finally by Red Clay; and these are overlain first by shallower-water foraminiferal ooze and then by coral-reef rock. In addition, the oceanic Echini confirm the depth at which the oozes formed. This great subsidence, if such it actually is, took place essentially during Miocene time, and in the Pliocene Barbados rose again into shallow waters. Vaughan thinks that the subsidence may have been to about 5,000 feet. On the other hand, T. C. Chamberlin (*Jour. Geol.,* 22: 141–143, 1914) advances the "alternative assumption that the benthos life and correlated conditions were carried up to unusual levels by upwelling currents about the island after it reached the stage of moderate submersion." The carrying "up to unusual levels by upwelling currents" is possible, for such occurrences are known in the present Pacific off South America, but one wonders if the abyssal Echini could have lived in "moderate depths." For the present the conclusion appears warranted that Barbados and the whole of the north margin of Paria during the Miocene had sunk to a depth of at least 5,000 feet, and that Barbados rose a similar amount during the Pliocene, and 1,100 feet higher during the Pleistocene, this being the present highest stand of the island. That Paria was very mobile in Miocene time is attested by the 14,000 feet of strata of that age in Trinidad and northern Venezuela.

The Bahamas. In speaking of the flat Gulf of Mexico plate, it was said that the Bahamas were but a continuation of Florida. It may be added that the southeastern Bahamas, and those of the medial region facing the Atlantic as well, appear to be volcanic additions now covered deeply with limestone deposits. Their submarine slopes show this, since they are much like those of the Bermudas, which have proved to be limestone caps on a subsided volcano that also grew up from the ocean bottom. If the outer Bahama islands have no volcanic bottoms, then we must assume that the steep and irregular submarine slopes are organic reef-growths on this now deeply subsided part of the plate. Of the two hypotheses, we prefer that of volcanic addition. On the other hand, the Florida-Bahama plate does not appear ever to have had dry land connection with the Antilles.

Times of diastrophism. In the Central American-Antillean region there were at least four times of major mountain-making, three of folding and one of faulting with local folding. (1) The oldest and most obscure one appears to be of Proterozoic time. Here the basement rocks are largely granites and highly metamorphosed sedimentaries that were peneplained before the late Paleozoic seas transgressed across them. When the marine invasion of Paleozoic time began is not known, but beneath the dated formations of Upper Carboniferous and Permian times there are others some thousands of feet thick, and it may therefore well be that marine Devonian is present, as is often assumed. As no Devonian is known in eastern Mexico, where the oldest Paleozoic strata are of Lower Carboniferous time, it appears more natural, however, to assume that the marine invasion of northern Central America was also of this time.

(2) The second orogeny took place in Central America after the deposition of the Lower Permian and evidently in the middle of late Permian time, since no marine formations of Triassic age are known anywhere other than in northern Mexico, where they are of the Upper Triassic. It is also fairly certain that orogeny took place in late Paleozoic time in western and central Cuba. Even though these mountains of Cuba do not connect with Central America, the late Paleozoic east-west mountain ranges of British Honduras, as stated by Sapper and others, do appear to have continued into the Caymans and southeastern Cuba, while those of Guatemala and Honduras extended across the Honduran-Jamaican banks into the island of Jamaica.

(3) Late in Cretaceous time began the third and best-known orogenic deformation. This was of great strength, with the foldings very extended, and along much the same lines as those of the late Paleozoic. It appears to have come to a conclusion before middle Eocene time. On the other hand, it is very probable that the mountain ranges of British Honduras now made the dominant anticlinorium, which appears to have extended unbroken across the Antillean Sea through the Caymans into southeastern Cuba, thence striking diagonally across Haiti-Santo Domingo into Porto Rico and the Virgins, and finally terminating in Anguilla and Barbuda. It is convex to the north. Another but subsidiary anticlinorium extended through southern Guatemala into northern Honduras

and across the Honduran banks, but may not have attained Jamaica. Between these anticlinoria lay a valley that during the late Cenozoic subsided beneath the sea and in Pliocene time was faulted into the present Bartlett Deep.

(4) The fourth diastrophic time began late in the Miocene and was of greatest force during the Pliocene, bringing on the present high stand of the Central American-Antillean lands and the overdeepening of the Caribbean Sea. These crustal movements were mainly of the epeirogenic kind, very extensive block-faultings with local compensating foldings.

Long separation of North and South America. About a century ago Humboldt taught that "The two American continents are practically dominated by a continuous Cordilleran system, running like a backbone through South, Central, and North America, connecting the whole western border of the hemisphere into one great mountain system" (Hill, 1898, pp. 158–161). About half a century later, Suess modified Humboldt's view of the American Cordillera, holding that the mountain chains of Mexico run into those of Oaxaca, and there, turning east through Guatemala and Honduras, continue through the Greater Antilles, finally swing south with the strike of the Lesser Antilles, and are joined to the east-west ranges of Venezuela, which are but a spur of the Andean system. Sapper, on the other hand, between the years 1894 and 1905, showed that the mountains of northern Central America trend mainly east and west, while those of southern Central America are much younger and of a totally different origin, namely, volcanic. Hill, better than any one before him, brings out the fact that the north-south trending Rocky Mountains cease in the "great scarp" of Mexico to the north of Tehuantepec; that the similarly striking Andes terminate in northwestern Colombia and eastern Panama; and that all the structure lines of northern Central America, the Greater Antilles, the Caribbean Sea, and the north coast of Venezuela are transverse to the great northern and southern chains of mountains, since their average strike is east and west. This great orogenic system, which Hill calls the Antillean system, makes the highland frame for the ancient Caribbean mediterranean on the north and south. The reason why this east-west trending has not been fully appreciated earlier, Hill states, is due "to the overwhelming proportions of the adjacent volcanic mountains." These latter developed in Cenozoic time "diagonally across the western ends of the east and west folds of the Caribbean configurations," in one series fringing the Pacific side of Chiapas, Guatemala and Honduras, and in another western Nicaragua and all of Costa Rica and Panama.

PALEOGEOGRAPHY

With this presentation of the broader geological facts and conclusions, we may turn now to the paleogeography of the Central American-Antillean region, but without reproducing here the nine paleogeographic maps. These will be published in the *Bulletin* of the Geological Society of America.

Late Paleozoic time. Back of the Upper Carboniferous, the paleogeography of the Antillean region is obscure and will be considered at another time. Then North America continued more broadly south into Central America, which in turn continued eastward into the Greater Antilles, at least as far as Porto Rico. It should, however, be kept clearly in mind that it is only since Cretaceous times that the Gulf of Mexico has attained its present great size and depth. Accordingly, in Paleozoic time the southern part of the North American platform was broadly rounded and bounded on the south by a mountain range that terminated eastwardly in the Antillean peninsula. Over this part of the platform the Atlantic Ocean spread shallow-water seas from time to time, but to the south of the mountain front lay the deep Pacific Ocean and the Caribbean mediterranean. South of the latter waterway was the Parian borderland of South America. There was then no Costa Rica-Panama and Lesser Antilles, and Florida and the Bahamas were of the flat-lying plate or foreland of Antillea. After Lower Permian time the whole of the Central American-Antillean folded land was again in the throes of decided mountain-making whose trends were in general east-west but curved to the south.

Jurassic time. Following the Upper Permian orogeny there was little change in the geography of the Antillean region, other than in its topography, during the whole of Triassic time. Early in the Jurassic, however, Atlantic waters were again invading most of eastern Mexico more and more widely, and finally in the middle part of this period the same sea began to spread across western Cuba, attaining its maximum late in the Jurassic both in Cuba and Mexico. At this time or shortly afterward, the eastern end of the Antillean geanticline began to grow submarine volcanoes, and more and more of them developed to the east with time, rearing their cones as island masses.

Cretaceous time. With the early Cretaceous, a tremendous change was inaugurated by the widespread and deep subsidence beginning in the western part of the Gulf of Mexico plate, and in consequence inundating widely most of Mexico, Texas and the northern portions of Central America. There also appeared for the first time the Venezuelan geosyn-

cline, which connected in the southwest with the far greater and older Andean one. Closure of the Caribbean opening into the Pacific appears to have been begun early in the Cretaceous by the elevation of a submarine ridge replete with volcanoes.

The Cretaceous flood attained its greatest spread in the north during the later part of the period, spreading far through western North America, over the Gulf border states as far as Illinois, and narrowly along the Atlantic border as far as Massachusetts. The Cretaceous flood also spread widely over the area of the Greater Antilles, especially in Cuba, Jamaica, Santo Domingo and Porto Rico, and in South America the earlier inundation was maintained. At the same time the western side of the Rocky Mountains was rising and pushing the marine invasion farther to the east, as is attested by emergence not only in the United States, but all through western Mexico and Central America as well. The Costa Rica-Panama land-bridge may have been completed before the close of the Cretaceous.

Early Eocene time. Long before the close of the Cretaceous, mountain-making had begun in Mexico, and later, crustal folding and elevation became general throughout Central America and the Greater Antilles, out to the eastern end of the Antillean arc. This dominant anticlinorium, which is curved to the north, extended through British Honduras and thence across the Caymans through southeastern Cuba, striking diagonally over Santo Domingo through medial Porto Rico and the Virgin Islands; farther east the submerged part of the ridge was studded with volcanoes, now the northern or limestone islands of the Lesser Antilles. Another but lesser anticlinorium, curved, however, to the south, extended through southern Guatemala and northern Honduras, and died out across the Honduran banks. Many other Laramides came into existence in Colombia during the late Cretaceous, being the northern terminations of the Andean system, with a northeastern spur that finally strikes east and west through northern Venezuela.

Apparently all the Greater Antilles with the exception of western Jamaica were emergent during early Eocene time, when the Laramide movements were completed. The same appears to have been true of the Costa Rica-Panama land-bridge, though much volcanic activity was general to this region well past the middle of the Eocene. The Bermudas are believed to have been emergent islands in Eocene time, with their volcanic activity exhausted before the close of this epoch.

Upper Eocene time. In Upper Eocene time, when submergence was again very widespread in the Gulf border states, the Greater Antilles were also more or less inundated, affecting Jamaica, Cuba and Haiti most, and Porto Rico only a little along the south shore. Colombia and Venezuela were likewise widely in flood, and most of Costa Rica as well, but Panama only marginally. Barbados was also submerged, and apparently all of the submarine volcanoes of the Caribbee arc north of St. Vincent had grown above sea-level into islands.

Oligocene time. The flood inaugurated in the late Eocene was most widespread during the Oligocene in the Greater Antilles and northern Guatemala, and especially so in Cuba and Jamaica. The Costa Rica-Panama land-bridge was almost everywhere covered by a shallow sea again widely uniting the Caribbean Sea with the Pacific Ocean. Barbados was emergent, and Florida appears as an island. The Bermudas, on the other hand, had gone beneath sea-level.

Miocene time. The Oligocene flood is continued locally—as in northern Guatemala and in Haiti-Santo Domingo—into Lower Miocene time, but in general this epoch is a time of emergence, becoming more and more so until a climax is attained in the Pliocene. Costa Rica is still completely submerged in the early Miocene, but in the middle portion of this epoch becomes emergent, after which time only the Caribbean shore of the entire connecting land is under water. Northern South America is still widely submerged in Miocene times, when very thick clastic formations were laid down. Barbados may have gone down 5,000 feet or more, and in keeping with this great subsidence is the very great loading of Paria with detritus. Florida has now made its appearance as a peninsula. The Bahamas may have been above sea-level in late Miocene time, but more probably during the Pliocene. The Bermudas also appear to be emergent.

Pliocene time. The Pliocene is almost everywhere the time of widest and highest emergence, and throughout Antillea there is tremendous fracturing and downfaulting. The old synclinal valley between Cuba and Jamaica—a sea since the Oligocene—is faulted into the Bartlett Deep, and Yucatan is deeply separated from Cuba, giving rise to the Antillean Sea. Eastern Honduras and Nicaragua and their banks are also let down to below sea-level, but Barbados has risen to near sea-level.

The marine overlaps of Pliocene time are small and marginal, the exception being the south side of the Gulf of Mexico where these marine waters flood all of Yucatan, northern British Honduras, northern Guatemala and the marginal states of southern Mexico north to Tampico; and probably during the late Miocene, but certainly in the early Pliocene, the

Gulf of Mexico for the first time communicated freely with the Pacific Ocean through the Tehuantepec portal. After middle Pliocene time this portal also was closed, and the whole of Central America has remained ever since an emergent and rising area. The Bermudas were submerged for at least a part of Pliocene time.

CHARLES SCHUCHERT

YALE UNIVERSITY

THE BIOLOGICAL ARTICLE AND THE OBLIGATIONS OF ITS AUTHOR[1]

AN address on an occasion like this gives one an opportunity to present some phase of current work that he deems of general interest or to discuss questions of concern to the group. Most of us would be inclined to agree that it is much more interesting to find out new facts and to discuss them with others than it is to give attention to questions of writing or publication. Having but recently lifted my head above an accumulated mass of manuscripts, the impression is strong in my mind of the evident weaknesses in our methods of publication and of the possibilities for improvement. The problem of methods of communication between investigators has existed since the beginning of scientific work. Merely as a matter of record, one writing is possibly sufficient, but to spread information of discoveries most profitably requires multiple copies. It was easy in the time of Leeuwenhoeck for him to sit down and indite a letter to the Royal Society describing his discovery of the new organisms in infusions and his delight and wonder in them. A natural forward step from this was for organizations to commit to the printed page a record of the discoveries of their members. In fact, all our means of communication are the result of these spontaneous responses to the necessities of the moment. We accordingly have organs of societies which represent the efforts of a limited group; we have journals which have been established through the energy and enterprise of individuals; there are the publications of research institutions which have been developed in response to their needs for recording and disseminating results of their studies and, finally, there are those journals which have been established by commercial houses which have, either through interest in scientific work or through a belief in the value of advertising, seen the advantage of attaching their names to serials.

Science is so new and of so rapid a development that its procedures are still largely empirical and only roughly adapted to the present scope and complexity of the field. This is particularly true of our publications. Some have a long and honorable history, largely because of fortunate connections. Many have lapses and others have changed relations or subject-matter and so have survived. Most of our old journals are those having scientific connections either with incorporated societies or institutes, or those with business associations. All these experiences are natural under the circumstances and present valuable suggestions for future conduct if carefully studied. As scientists we should feel the challenge to take stock of these experiences and to devise means for getting our communications shaped to modern requirements and for providing an effective and convenient system of journals to meet the needs of all groups of biologists and all aspects of the subject.

The fundamental question which we have to consider is: Are publications incidental in their relations to biological progress and so to be left unconsidered, or are they essential elements of the program and deserving of careful planning and management? In the developmental stage of our subject it was natural that progress should be tentative and without comprehensive plans. In its present state, has not the time arrived for careful study and planning? Here we have to consider whether anything else can be done to improve the service of our journals without sacrificing the essential freedom of investigators to work in the manner best adapted to produce results.

If we hope to improve the character of scientific papers it is imperative that we give thought at the same time to where they are to appear, for the character of the article depends in part at least upon the medium available for its publication. The problem then is to discover the course which will make most easy and profitable the use of written records of our discoveries and which will run the least risk of smothering individual initiative and opportunity.

As a necessary preliminary to any future action we must stop to consider our present situation. In doing this we find that there are usually produced about 40,000 titles annually, scattered through some seven or eight thousand periodicals and filling perhaps 500,000 pages. The consideration suggests itself that while this is a staggering total, there are represented a great variety of subjects, so that the individual worker with limited interests is not necessarily concerned with the whole output. While this is certainly true it is also evident that with a growing output there inevitably follows increasing personal limitation of contacts, because each of us has but a limited time to give to reading, and the more that is employed in searching the less there remains for actual reading. While the subject itself is rapidly broadening and ex-

[1] Address delivered before Section F—Zoology, of the American Association for the Advancement of Science, New York, N. Y., Friday evening, December 28, 1928.

tending its scope, the individual becomes increasingly narrower and more specialized.

To older workers who have grown up in the subject or to those who have not been obliged through experience to consider the problem in its entirety, the intensity and growing seriousness of the problem is not so evident. The exceptionally placed or fortunately endowed person can still maintain his connection with his field, but the beginner and those less fortunate are in distress. Looking also beyond the present and considering our responsibilities towards those who follow us, we have other reasons for giving serious thought to the nature of our written work.

A review of the journals that now serve us indicates that they have some definite limitations. They are often not definitely representative of the subjects indicated in their titles. They overlap the field of other journals on one hand and neglect phases of work still uncared for. Taken altogether they do not cover the whole field. Some subjects are provided with many outlets; others with few or none. In management they tend to be irresponsible and sometimes inefficient. Generally they are inadequately supported, which sometimes results in their early extinction. Doubtless in many cases this is an advantage, but may result in serious inconvenience to those served. The quality of the make-up and illustrations inclines to be poor. If the price is low enough for individual workers to subscribe, the quality is indifferent and the life of the publication short. When, on the contrary, the journal is in the hands of a publishing house which desires to make it profitable, the price is so high that the individual worker can not afford to possess it. In few cases do our journals conform to the ideals of what a scientific journal should be.

When we turn to the consideration of the articles which appear in these journals, many of us would be inclined to agree that they possess some or all of the following defects:

(1) The author takes a wrong attitude in writing, so that frequently this is highly subjective and indicates most clearly that the facts and impressions have been set down by the author, not with the idea of informing his fellow workers of the nature of his observations and conclusions, but rather as a record of his own impressions. I have the conviction from much practical experience that if our investigators would give primary consideration to the needs of their readers a very great reduction in the volume of literature would result. Very frequently in an editorial capacity I have found it possible to persuade authors to reduce the length of their articles as much as 50 per cent. merely by suggesting that they be written for the reader rather than for the writer.

(2) When we compare biological articles with those written by chemists and physicists we are impressed by the evident verbosity in style and redundancy of detail observable in biological papers. Frequently there is over-much historical survey and a multiplicity of quoted opinions which are entirely unnecessary for clear exposition. Excessive and expensive tabular data of interest to only a limited few also encumber many articles. Repetitions of already available bibliographies and the inclusion of unnecessary illustrations are not uncommon faults. Mere repetition of facts with unessential differences in detail from those recorded in previous publications multiplies many pages.

In considering possible improvements it is recognized as indisputable that there should be opportunity for investigators to publish whatever views they may have, that there should be no compulsion to adopt any particular form or attitude in treatment of their results, and that agreements of majorities should not be allowed to cripple the ambition or initiative of individuals. However, we must recognize that complete anarchy is neither possible nor desirable and that restrictions are inevitable and necessary. Such restrictions are, however, best imposed according to the judgment of representative opinion rather than according to individual whim. It is inevitable that under any circumstances the opinion of leaders will have disproportionate weight. In considering the rights and privileges of authors it should always be remembered that there is an essential difference between freedom to work without restriction and freedom to publish *ad libitum*.

Also to place limitations upon articles submitted to journals having definite objectives is not to close all opportunities for publication. The policy of restrictions in journals is one long established. The mere fact that a particular field is chosen limits the range of included articles, and to designate, in general terms, their form and extent is only a slight step further. At some point or other the needs or desires of the individual are sure to conflict with the interests of his group. When this point is reached the one must of necessity give way to the many. There is undoubtedly an educational value for young investigators in the writing up of their investigations and it is held by some to be a function of our journals to supply this training. It may well be doubted, however, whether it is the obligation of the editor, already overwhelmed by his own duties, to contribute thus to the operations of others. Rather it should be the obligation of those in charge of laboratories to see that articles coming from them should be expressed in dignified, effective and understandable language. It is no more appropriate to put the responsibility for judging the form and character of an article upon the shoulders of an editor than it is to hold him accountable for the character of the work described.

very properly he should be asked to judge the availability of the paper for his journal and to prescribe its length according to ready funds, but he should not be required to assist in its production.

The author does not stand alone. It is recognized that to a large and increasing degree investigators are becoming mutually interdependent and that with the increase of such interdependence the responsibility of the individual to his fellows is correspondingly augmented. This implies that, in the nature of the case, restrictions, self-imposed or by external compulsion, must result. While practically unrestricted publication through separate brochures and books is possible, the introduction of an article into the journal of a society may come only through conforming to certain general tenets and rules, often ill-defined, but generally recognized. Unfortunately the cost of publication sometimes imposes additional restrictions not always desirable in character.

In view of this evident mutual dependence of investigators, it might well become the author to inquire rather closely on two points. If he contemplates making a permanent addition to the literature on any subject which will in the course of time be consulted by many other workers and repeatedly referred to in bibliographies, it is only just that he should seriously consider whether he has anything in the way of new facts or pertinent generalizations to add to our knowledge of the subject. It is probably all too true, as is frequently urged, that there has been undue pressure brought to bear, especially upon young investigators, to publish in order that they might bring their names before the scientific public. As a corrective measure some have even gone so far as to suggest the organization of societies for the prevention of publication, but I think we would all agree that this is extreme. Even the mildest of us would, however, be inclined to agree that the author owes it to his fellows to consider carefully the value of what he proposes to contribute. If, after this self-searching, he still feels that his contribution may be worthy, he has next to consider how it should be presented so as most readily and fully to serve the largest number of interested workers. Certainly it is a truism to say that neither the number nor the length of articles should be the measure of service, but rather the quality of the results and the form of their presentation.

In the search for light upon the character and form of his contribution the author certainly should feel that those with whom he is immediately associated have some interest in what he proposes to do, and in most well-regulated departments he should feel free and even impelled to get the judgment of his fellows. It is not easy for one to be assured of the value of his own production and it is often an invaluable help to have some one intimately associated with him to pass judgment upon his work.

When the author has done his best in these ways and has the article ready for presentation, the responsibility as to its future disposition often devolves upon the organization which supports the journal in which it is proposed to publish. Obviously such an organization or its representatives must judge whether the type of article is appropriate to the journal and may even go further and ascertain whether it is in the form which would make it a most worthy contribution. As the matter now stands, in many instances the group representative must proceed even further and judge the quality of the article or even its literary form. The appearance of an article in a journal sponsored by a society would imply that it does not do violence to the current opinion of the group, but would to that general degree carry with it the support of the group. For this reason the organization is entirely justified in exercising such a measure of censorship as will insure a reasonable conformation to accepted views.

In considering possible needs in our publication situation I do not desire to indicate in detail what should be the character of biological articles or what the attitude of author or authors or the responsibility of the organization involved, but rather to suggest how this may be determined and defined. In approaching this aspect of our subject I am forcibly reminded of the opinion of Matthew Arnold expressed in his "Essays in Criticism," when he considered the possibility of establishing in England an academy similar to the French Academy. He says:

> Such an effort to set up a recognized authority imposing on us a high standard in matters of intellect and taste, has many enemies in human nature. We all of us like to go our own way and not be forced out of the atmosphere of the commonplace habitual to most of us. We like to be suffered to lie comfortably in the old straw of our habits, especially our intellectual habits, even though the straw may not be very fine and clean.

There are so many things to be considered when we approach a problem of this character. In most cases we wish to be assured in some manner or other that the substance of the article is worthy of record, but there are those amongst us, blithe spirits who play with words in so pleasing a manner that we read what they have to say not for what it means primarily but simply for the pleasure we find in the manner of expression, and we would not wish to deprive ourselves of such pleasure by any rigid determinism.

If we desired only the immediate improvement of our scientific output, possibly the best way would be

to provide a benevolent despotism consisting of the best editors we could find and placing entire responsibility in their hands. But if we are concerned with the eventual and permanent improvement of our product, we would doubtless find here, as in other cases of absolutism, that the effort for immediate improvement results in eventual decadence. We have to look towards the future and the development and improvement of the coming generation. We would therefore feel safer in adopting such measures as will insure general study and thought by as many of the human element as may be drawn into such consideration. It is education and not compulsion that we would best choose.

We have here occasion to consider again the two aspects of the program: (1) to develop in the mind of investigators through discussion and personal consideration a working conception of what should be the form and content of a biological paper of a particular type; and (2) through organization to plan a system of publications which shall care for all phases of work in the field, and shall care for different types of writing—original articles, reviews, abstracts, etc.—and indicate the best form for each of these.

As to the means for effecting improvements in articles, I imagine each one of us would have his own opinions. After giving the subject much thought it seems to me that at least one practical way of proceeding would be somewhat as follows: First, to secure the interest and attention of those most familiar with the problem; second, to spread the consideration of publication questions by involving numbers of workers in different organizations through committees appointed by societies to study them; third, to develop these opinions further by bringing together representatives in various groups for comparison of views; fourth, after some consensus of opinion has been reached, to formulate a general statement regarding the form and character of biological articles which might serve as a guide, but not a rule, for others; fifth, to submit such a statement to responsible organizations for criticism and consideration; and, sixth, perhaps finally to publish the statement as a majority opinion which might be used in a variety of ways.

In outlining the question of improving the character of our journals we might undertake careful studies to discover first, the various types of publications required; second, to find the best means of conducting and managing these; and, third, to obtain an appreciation of the relative responsibilities of the author, school or department, society and other organization involved. Any program decided upon should recognize that our situation is not static, but that it often changes rapidly so that any program of improvement should be flexible. Also in view of the established position of existing journals, the application of any program of improvement should be gradual, taking advantage of opportunities and allowing fully for the work of individuals who have had the energy and foresight to provide means of record and communication for others.

When we attempt to go into the question of determining the precise function of a biological article, at first sight it might seem easy, but careful consideration reveals that such articles may do many things. Among these obviously they supply a permanent record of new facts. They reveal the investigator's interpretations of these facts. They at the same time serve to clarify and unify the author's views. They also explain the methods by which results were obtained, and to the discriminating reveal the author's aims and attitude. In the case of beginners they afford training in methods of exposition. Among these possibilities may occur conflict between the welfare of the author and that of his readers. In most cases it would seem quite right and proper that the good of the many should transcend that of the one. We must recall here again the weighty fact that there are many who come after us, and, considering all contrasting claims, the case of the many as opposed to the one grows in importance with the years.

There are those who consider the rights of the author paramount, the claim being that the results of an investigation belong to the author and are his to dispose of, but this is not entirely true, for he owes his opportunities in most cases to institutions which pay him for his time, provide him with facilities for work and are judged by its quality in relation to similar work from other institutions. He is largely indebted also to his fellow workers who have in the first place introduced him to the subject, trained him in its methods and provided him with the background against which he works. But, even assuming that the author is a creator of values that, *ipso facto*, adhere to him, he automatically relinquishes his exclusive rights when he passes his results on to his fellows by publication. He must consider how his contribution should be built into the common edifice, and he can not demand that he be allowed to dispose of it regardless of the established plan. In attempting to evaluate the various functions of an article we must consider how they stand in relative importance and, in the event of sacrifice, determine which should go first. If the apparent rights of the author come into conflict with those of his fellows, and he be willing

o cast aside the restraints of group purpose, as con-ivably he might be justified in doing, he always has e recourse of independent publication. This will sure him a hearing without involving others in sponsibility for his views.

Second-hand book shops carry as dead stock on eir shelves many appeals from decisions adverse to cepted tenets. In these times it would be an ex-edingly rare occurrence for a major injustice to be ne in this way.

If an editor, from painful experience, be permitted suggest some detailed matters in which our bio-gical articles might be improved, I may mention veral.

There is the general form of treatment which any rticular article should have. The growing convic-on that commonly this takes the subjective rather an the objective obtrudes itself. A comparison tween an average run of biological articles and a milar series in the physical sciences strongly con-rms this suspicion. Undoubtedly clarity of presen-tion and definiteness and serviceability of the facts resented would be suggested if a plain and simple atement of the facts involved were made. We ve to consider here not the mere question of brevity, t serviceability. Brevity may be the soul of wit, t it is not always the best form in which to embody idea. Papers should be long enough to present early the situation described, but no longer. Brev-y is not an end in itself, but only a means to greater recision of statement and convenience of use.

The oft-repeated criticism of the style of English und in scientific articles may trace back to faulty alysis and planning rather than to misuse of words r constructions. We may not be justified in demand-g that the articles we read be entertaining, but at ast we can ask that they be clearly informative.

Perhaps it is too far a cry to enter into the question the author's attitude, and fortunately this is not much required as might formerly have been the se, but one is tempted to express the opinion again at matters of priority and personal credit are not r primary concern, but rather the advancement of e science in which we choose to labor.

We might also wish, speaking editorially, that thors would give more thought to the type and the mber of illustrations which they utilize. Not in-equently it is asked that we draw strongly on our mited funds for the purpose of repeating uselessly e same type of figure with minor variations, hereas one clear example would be entirely sufficient. seems not to be fully realized that illustrations are e author's interpretations and that mere repetition the same conditions will not add to the objectivity the fact.

After wading through repeated historical surveys of the same topic in a series of papers, one is tempted to wish that this phase of presentation might suffer an eclipse. Undoubtedly there is need for this criti-cal treatment of accumulated facts, but there ought to be provision for such in a type of publication devoted exclusively to it. Similarly, the constant repetition of the same material in bibliographies leads to the suggestion that reference to existing compre-hensive bibliographies might often take the place of these repetitions. It would be an added convenience if there were some reasonable standard in the form that references should take.

These are only a few of the many features of editorial work which are the constant experience of those who have been drafted to function in this capacity. Should one wish to go further than mere palliative measures and indulge in Utopian dreams of what might best be done for our ultimate benefit, there is suggested the comprehensive plan by group action of a system of journals in which there would be provision for covering the biological field by journals with definite limitations of scope and treat-ment. As it is now, the same general subject, such as cytology or genetics, may be found represented in a large number and variety of journals so that it is beyond the possibility of an individual investigator to possess the serial literature in any subject, and it is growing to be the case that even libraries are embarrassed by the number of journals and the dis-tribution of articles within them. It would be of greatest value if our materials could be definitely segregated according to the needs of existing groups. This would greatly facilitate the matter of reference and would even make possible individual possession of the main sources of information. We need also a selection of materials according to the treatment accorded them. We should have journals in which brief articles within a fairly limited field might be found. There should be other journals where longer original articles might find a place. Also there should be publications of a monographic type which are expensive in production and limited in distribution. We need also reviews of various types and that more highly developed and difficult type of treatment which involves the critical faculty in a large measure.

The growing need for a ready and brief introduc-tion to the literature through abstracting systems has progressed further with us and has found measurable realization. So far, such progress as we have made in perfecting our publication facilities has been of an uncertain and incidental character. It does not seem that we can hope for great progress until we have made this feature of our work a major problem and have given it some such degree of consideration as is

involved in measures required to secure the facts which are to be recorded.

Any one with experience in biological publications must realize the complexity and difficulty involved and so would hesitate to suggest a definite remedial plan. Certainly I have no intention of presenting one. What I would plead for is a sustained, careful study of the problem with the gradual introduction of such features as from time to time justify themselves. Our societies are not by nature well adapted to the execution of concrete projects, but for the study of such questions as the means of making most available and useful the common store of knowledge in a subject it seems to me they are peculiarly suited. Is it too much to hope that our biological organization may meet constructively what, to many of us, seems to be an approaching crisis in a major department of our activities?

Clarence E. McClung
University of Pennsylvania

HENRY BURCHARD FINE

It is with profound regret and a sense of the loss which has been sustained by the academic world that we record the death, on December 22, 1928, of Dean Henry Burchard Fine, of Princeton. In the evening of the previous day, the bicycle which he was riding was struck by a motor car and he was thrown with such violence to the roadway that his skull was fractured. He never recovered consciousness and died early next morning. He was in his seventy-first year, but in full mental and physical vigor, and there seemed every prospect that many years of usefulness still lay ahead of him.

Fine had been connected with Princeton University either as student or teacher for over fifty years. He entered as a freshman in 1876, and at once took a leading place in his class and eventually in the undergraduate body, by the force of his intellect and the vigor of his personality. As happens so often, his first interests were not those which afterwards attracted him. In his earlier college days he was an ardent student of the classical languages, and when he took up the study of Sanskrit the career of a philologian seemed marked out for him. Later in his course, however, largely owing to the influence of George Bruce Halsted, who had a very stimulating effect on the men who came in contact with him, he was attracted to the exact logic of mathematics. After a graduate year spent as fellow in physics, he was appointed an instructor in mathematics. He spent a year and a half, on leave of absence, in Leipzig, working under Felix Klein, and there took his doctor's degree in 1885, with a thesis on a subject connected with Grassmann's "Ausdehnungslehre." His interest in the foundations of mathematics, which was heightened by this investigation, bore fruit a few years later in the publication of his "Number System of Algebra," a little book which was perhaps not suited to the general run of elementary students, but was extremely fascinating to those who were interested in abstract thinking. The same accuracy of logic and the same instinct for perfection of statement appear in the two text-books on algebra and the calculus, which were published later in his career, the last one in fact appearing only the year before his death. In 1891 Fine was appointed to the Dod professorship of mathematics, and from that time on his position as leader of the mathematical department in Princeton was recognized. It was his settled policy to introduce into that department only men of proved abilities in research, and under his guidance the department has been continually strengthened and its productivity increased. Whatever reputation Princeton now has as a center of mathematical activity is due largely to his firmness of purpose and his wisdom in the choice of men.

Fine was a member of the American Mathematical Society from its foundation, and for a term served as its president. He was also a member of the American Philosophical Society.

As an administrator Fine rendered great service to his university. In 1903, early in the presidency of Woodrow Wilson, he became dean of the faculty, with charge of the scholarship and discipline of the undergraduates. He introduced the policy of establishing fixed rules governing the standards of scholarship, which were generally recognized as reasonable, and of enforcing these rules rigorously and almost automatically. This policy has been followed ever since with good effect. From time to time, as the opportunity offered itself, the standards have been raised, but the method of administering them has remained unaltered. Fine's impartiality and his sympathy with other men's points of view contributed to his success in the establishment and development of his policy.

In 1909 Fine was given by President Wilson the task of supervising the development of the school of engineering, and the organization of the various scientific departments, with the title of dean of the scientific departments. He retained this position when he resigned the deanship of the faculty, and by his initiative and helpful advice he was largely instrumental in the development which those departments have had in recent years. He had an important part in the negotiations with the General Education Board which resulted in the gift to Princeton University from the board of one million dollars for the endowment of research in science, and the additional two million

ollars which had to be raised as a condition of receiv-ng this gift was very largely contributed by personal riends of his, as a token of the confidence and affec-ion which he had inspired in them. He had the satis-action before his death of seeing this fund completed.

Fine was one of the type of scholar developed efore the days of extreme specialization. While he as familiar with the work that was being done by he other members of his department, and while he ept abreast of the general progress of mathematical cience, he did not forget his earlier interest in the lassics and philosophy. He was well read in general iterature, and had an excellent style in writing. He ad valuable and well-grounded opinions on all ques-ions of university policy and of general education. e enjoyed the students' sports, and for nearly the hole of his professional career served on the univer-ity committee on athletics. In his early days he layed well on the flute, and he had throughout his ife an extensive knowledge and a fine critical judg-ment of music.

In all matters to which he gave his attention he was clear thinker, a most persuasive though not a ready ebater, and an excellent negotiator. If he had ac-epted the post of ambassador to Germany which was ffered him by President Wilson he would have shown n a larger stage that he had the qualities of a great iplomat. In personal intercourse he was singularly enial and winning. His circle of friends, both old nd new, was large and bound to him by sincere af-fection. He will be long mourned by those who loved im and have looked to him as an example of the ighest type of scholar and teacher.

W. F. M.

PRINCETON UNIVERSITY

HARRISON GRAY DYAR

A MAN died in Washington on January 21 who was nown personally to comparatively few scientific men lthough his work had made him famous in a growing eld. Dr. Dyar was born in New York City, Feb-ruary 14, 1866. His father was a famous inventor, ho is said to have disputed the priority of S. F. B. Morse's invention of the electric telegraph, and who ndoubtedly made a fortune by inventions relating to yes. Young Dyar was educated at the Roxbury atin School, at the Massachusetts Institute of Tech-ology and at Columbia University. He took his achelor's degree at the Massachusetts Institute in 889. In 1893 he returned and took the last year f its biology course, going the next summer to Woods Hole. He then went to Columbia University, gaining is A.M. in 1894 and his Ph.D. in 1895. In 1894 he ublished an important paper on the classification of Lepidopterous larvae, and in 1895, after research work in bacteriology, he published his thesis, which was entitled "On Certain Bacteria from the Area of New York City." This study pointed out certain things with regard to the supposed specificity of bac-teria and their variability which were not in accord-ance with the general opinion of that time. Recent writers, however, have changed the general view and refer to this early paper of Dyar's with distinct ap-proval.

His work on Lepidopterous larvae attracted the at-tention of entomologists, and, meeting him in 1897, the writer invited him to come to Washington. The invitation was accepted, and he became custodian of the collection of Lepidoptera in the U. S. National Museum, a position which he still held at his death. For a comparatively short period he was in charge of the whole of the insect collections of the museum.

When the attention of the world became turned to mosquitoes as the result of the discoveries concerning their disease-bearing function, he began to study mosquito larvae—at first because they were larvae and he had been studying intensely the larvae of another group of insects. Later he saw the importance of this stage to a thorough understanding of the Culicidae, and from this he became interested in everything about mosquitoes.

When the Carnegie Institution of Washington made its first grant to the present writer for the prepara-tion of a monograph on the mosquitoes of North and Central America and the West Indies, Dyar was chosen to do the work on the larvae; Frederick Knab was later associated, and the two in collaboration are mainly responsible for the taxonomic portions of the extensive four-volume work on this subject published from 1912 to 1917 by the Carnegie Institution.

Dyar's financial means were such that he was not hampered in his work by salary necessities, and dur-ing the major part of his thirty-one years of life in Washington he received no compensation for his work, although for a few years he was on the rolls of the Bureau of Entomology of the Department of Agri-culture. He was consequently able to take long field trips at his own expense, to investigate regions where field study was needed, and he thus became acquainted with local conditions over a vast extent of territory. He adopted in his taxonomic work the plan of intro-ducing synoptic tables of the larvae and of the male genitalia as well as the other structural features of the adult, and thus brought about very largely a fixity of classification little known in many other groups.

In the years between 1917 and 1927 there was great activity over the world in mosquito study. New forms came to light, not only in the regions included in the

scope of the Carnegie monograph, but in South America and in other parts of the world. A mosquito taxonomist of great ability appeared in the British Museum of Natural History (F. W. Edwards) and, working largely with the Old World fauna, he arrived at conclusions coinciding in the main with those reached by Dyar. With the incoming of material from South America, and with the publication of the excellent "Monograph of the Mosquitoes of Surinam," by the Bonnes, the necessity for a supplemental volume to the Carnegie monograph became apparent. The interest of Dr. J. C. Merriam was enlisted and the consent of the trustees of the Carnegie Institution was gained for the preparation of a volume to include the mosquitoes of all the Americas. Dr. Dyar was a tireless worker, and by the close of 1928 he had completed the volume and had seen it published.

All these years he had been publishing shorter articles, both on mosquitoes and on Lepidoptera, and two as yet unpublished papers were left in completed form.

Thus has ended a life of intense scientific activity and one which undoubtedly has made important contributions to human knowledge.

L. O. HOWARD

U. S. DEPARTMENT OF AGRICULTURE

SCIENTIFIC EVENTS

THE BREEDING OF BENEFICIAL PARASITES

A LABORATORY for breeding beneficial parasites, established by the British Empire Marketing Board and under the control of the Imperial Bureau of Entomology, has now been at work for rather more than a year. According to a report in the *Journal* of the American Medical Association, consignments of insects have been sent out in response to requests from Canada, New Zealand, Australia, South Africa, Kenya, the Falkland Islands and different parts of England. Between 20,000 and 30,000 larvae of the pine tortrix, 90 per cent. infected with a parasite that attacks it in its larval stage, were recently collected (largely from Brandon, in Suffolk) for Ontario. Ontario also received 20,000 parasites of the greenhouse white fly, which was exported on whole tomato sprigs and sent over in cold storage. Adult parasites of a scale insect that attacks fruit were sent over in small sealed test tubes to Vancouver and provided with raisins for nourishment in transit. The wood wasp *Sirex* infests most timber-growing countries. Its larvae bore their way into tree trunks, leaving behind them neat circular tunnels in the wood. The *Sirex* parasite is another fly, *Rhyssa,* with a long ovipositor which it thrusts right through the grain of the wood until it penetrates the body of the wood wasp lar on which it lays its egg. Three hundred and fif of these *Rhyssa* larvae have been collected in Dev and shipped to the Cawthron Institute in New Ze land. Australia and New Zealand have also receiv 30,000 larvae of the pear-slug infected with three sp cies of parasites, collected mainly from norther France. The sheep blow-fly, a big greenbottle, la its eggs in dirty and matted wool on living sheep, a the maggots that hatch out eat their way into the an mal's body. There is, however, a parasite which i turn lays its eggs in the blow-fly maggot and eventu ally kills it. Hundreds of thousands of these maggo with their appropriate parasites, are being bred at th laboratories, and some have already been exported i the chrysalis stage to Australia, South Africa and th Falkland Islands, where the blow-fly causes enormo loss of sheep life. Other recent exports include par sites of the woolly aphis of the apple, sent to Indi and Kenya Colony, and of the earwig, sent to Ne Zealand and Canada. Three Australian scientific me from the commonwealth department of entomolog are carrying out research at the laboratories unde the superintendent, Dr. Thompson, who is himself Canadian. One is working on the sheep maggot, o blow-fly, already mentioned; one on a parasite of th apple-ravaging codling-moth, and one on an inse that attacks a troublesome weed, Saint-John's-wor Dr. Myers, of the Farnham Royal staff, has gone t the West Indies to deal with tropical parasites, an he will organize shipments of beneficial insects be tween the various islands and British Guiana.

MINERAL PRODUCTION OF THE UNITED STATES IN 1928

THE total value of mineral production in the Unit States in 1928 was approximately $5,400,000,000, a estimated by the United States Bureau of Mines, De partment of Commerce. This is a decrease of ap proximately 2 per cent. of the total value of miner products in 1927 and is due almost entirely to a de crease in the total value of mineral fuels. Of the the quantity and value of coal decreased; the quanti of petroleum produced changed little, but the val decreased, and the quantity and value of natural g and natural gasoline increased as compared with 192 The total value of metallic products shows an increa due to increase in quantity and unit value of copp and an increase in the quantity of iron produc Decreases were shown for gold, silver, lead and zi The total value of non-metallic mineral products sho approximately no change. Decreases for some these products were offset by increases for others.

The following figures give the estimated total val of metallic mineral products and non-metallic miner

products other than fuels and of mineral fuels produced in the United States in 1928.

ESTIMATED VALUE OF MINERAL PRODUCTS OF THE UNITED STATES, 1928

Metallic	$1,260,000,000
Nonmetallic (other than fuels)	1,240,000,000
Mineral fuels	2,900,000,000
Total	$5,400,000,000

These estimates are subject to revision and replacement by precise figures as soon as the Bureau of Mines can complete the canvass of mineral industries just begun to obtain accurate statistics for the year 1928. In this canvass the bureau is sending to every mining, quarrying and well operating company an inquiry soliciting a report on the output of each mineral commodity by each producing establishment. Early success in this undertaking is dependent upon the continuation of the prompt and cordial response on the part of the mining companies which has been the basis of success in this statistical endeavor through many years.

THE AMERICAN PHILOSOPHICAL SOCIETY

ACCORDING to a statement sent from the society the 435 members were asked on February 1 to put in writing their views as to how the society can best carry out its program of intellectual stock-taking which was announced on January 12.

In the letter, Dr. Francis X. Dercum, president of the society, asked four questions and in making the letter public also gave out a statement setting forth what the stock-taking purposes to accomplish.

"The project of an intellectual stock-taking is as large an undertaking as we care to make it," Dr. Dercum wrote. "I believe that we can never approach finality; but I also believe that the society, which numbers among its membership leaders in all fields of intellectual advancement, is equipped to perform a distinctive service in promoting coordination of scientific and social efforts."

The questions incorporated in Dr. Dercum's letter are said to hold closely to the four tentative questions made public at the time of the announcement of the society's plan for an intellectual survey. They are as follows:

What to-day is the world's intellectual need?

Is there a drifting apart of the purely scientific interests and the humanistic interests?

Is there a loss of perspective and of grasp of fundamental principles by reason of specialization in education and in thought?

How can these interests and these branches of individualistic learning be coordinated into one program with one common purpose—the promotion of all useful knowledge?

The answers to these questions, Dr. Dercum believes, will prove an invaluable guide to the society in its stock-taking enterprise.

The present American members of the society, according to Dr. Dercum, are divided by profession into 26 groups representing as many branches of learning. These groups and the numbers of members in each is as follows:

Anatomists, 7; anthropologists, 5; archeologists, 3; astronomers, 25; authors, 6; botanists, 24; chemists, 36; classical and modern philologists, 12; educators, 20; electrical engineers, 11; engineers, 17; geographers, 6; geologists, 25; historians, 18; lawyers, 23; mathematicians, 13; men of affairs, 10; meteorologists, 1; orientalists and comparative philologists, 10; paleontologists, 8; physicians, 17; physicists, 32; physiologists, 10; political economists, 4; psychologists and philosophers, 6; zoologists, 30.

In explaining what the society hopes to accomplish by means of its survey, Dr. Dercum pointed out that "all through the ages scientists, humanists, theologians, economists and inventors have been adding to the world's store of useful knowledge but that due to a lack of coordination and loss of perspective this store of knowledge is not being fully utilized. For this reason this intellectual stock-taking is being undertaken to determine, by symposium and synthesis, how all this materialism, this specialization, economic and intellectual, these divergent scientific and social opinions, can be brought together for human advancement."

THE ECLIPSE EXPEDITION OF THE NAVAL OBSERVATORY

THE eclipse of the sun in May will be observed by an expedition under the auspices of the Naval Observatory, which sailed for the Philippines on January 28, on the naval transport *Chaumont*, from San Diego, California. Commander C. H. J. Keppler is in charge of the expedition, while the scientific work is under the direction of Professor Wilbur A. Cogshall, of the University of Indiana. Mrs. Cogshall accompanies him as scientific assistant. The staff of the Naval Observatory is represented by Mr. Paul Sollenberger, the technical supervisor of the observatory's time service and an expert observer.

Lieutenant H. C. Kellers, Medical Corps, U. S. N., surgeon, is also acting in behalf of the National Museum for the collection of specimens of fauna and flora in the vicinity of the eclipse site, as he did in Sumatra during the 1926 eclipse.

In addition to this information a statement from the Navy Department says that the expedition this year proposes to set up its apparatus in the vicinity of Iloilo in Panay and will receive the cooperation of the Manila Observatory in selecting the exact site. The Manila Observatory, under the direction of Father Selfa, is known for its work in the meteorology of the Philippine Islands.

A tender will be assigned for the use of the expedition upon its arrival in the Philippines, and also an expert motion picture photographer and the necessary mechanics and helpers will be attached to the party from the Asiatic Fleet.

This year's eclipse, which takes place during the afternoon of May 9, is of exceptional importance on account of its long duration, the maximum duration of totality being over five minutes. Although wholly invisible in the United States, in fact taking place in the dark hours of the eighth of May, the eclipse stretches its beam of darkness over widely separate land areas from the northwestern end of Sumatra, across the Malay State of Kedah, across Siam and southern Cambodia, and finally over the middle group of islands of the Philippines between Luzon to the north and Mindanao to the south, including the important cities of Iloilo, the second in size in the Philippines, and Cebu, where Magellan met his death in his round-the-world cruise.

The observatory's expedition, in addition to special observations on its own part, is duplicating certain features of the program arranged for the party from the Sproul Observatory of Swarthmore College which will go to Sumatra. An interesting comparison of data is in prospect, if both parties are favored with clear weather. Several other expeditions are to cover the many phases of this exceptional eclipse. British expeditions from Greenwich and Cambridge are in prospect. Four German expeditions are planned, one from Hamburg possibly operating in the Philippines. Then there are Dutch, French and Italian expeditions in preparation and possibly one from Australia.

The corona effects of this eclipse will not repeat themselves for another quarter of a century. It is, therefore, the corona that will receive intensive study this year. Besides a study of the Einstein problem, the various programs contemplate spectrophotometry of the chromosphere and corona both in the red and in the ultra-violet, a study of solar radiation near and through totality, experiments to test the effect of totality on radio transmission, relative intensities of the lines of the coronal spectrum, improved measurement of the wave-lengths for the coronal lines with a spectrograph of high dispersion, examination for displacement of the dark lines of the outer corona with a slit spectroscope of high dispersion, a study of coronal rotation with a falling plate spectrograph and interferometer and with a quartz spectrograph, and other features, including a special study of the shadow-band phenomenon.

THE HARVARD UNIVERSITY EXPEDITION TO STUDY TROPICAL MEDICINE IN YUCATAN

AN expedition which may throw new light upon the Mayan civilization started on February 1 for Yucatan from the department of tropical medicine of the Harvard Medical School and School of Public Health.

The immediate purpose of the expedition is to make a medical survey of the population of a section of that country. The Carnegie Foundation of Washington has appropriated funds and will bear a portion of the expense of the expedition.

The region selected is about Chichen Itza where is situated a famous sacrificial well of the aborigines. There are villages of mixed population in the neighborhood and others of practically pure-blooded Maya Indians. Little is known of the diseases of these people and it is hoped that the study may throw light upon the causes of the complete collapse of the Maya civilization which followed close upon the Spanish conquest.

The "Yucatan Medical Expedition" will have its headquarters among the ruins of the ancient city at Chichen Itza where, for some years, the Carnegie Foundation has maintained a station for archeological research. Mr. Sylvanus G. Morley, who is now on the ground, has charge of the station and of the archeological work being done there. Mr. A. V. Kidder, of Andover, made the preliminary arrangements with the Department of Tropical Medicine on behalf of the foundation.

The personnel of the Medical Expedition is as follows: Dr. George C. Shattuck, in charge; Dr. Joseph C. Bequaert, entomologist, and Dr. Jack H. Sandground, parasitologist, all of the Department of Tropical Medicine of the School of Public Health; Dr. Kenneth Goodner, bacteriologist, of the department of bacteriology of the Harvard Medical School, and Mr. Byron L. Bennett, laboratory technician.

THE ADMINISTRATION BUILDING OF THE DEPARTMENT OF AGRICULTURE

THE corner-stone of the main administration building of the Department of Agriculture in Washington was laid just before noon on Monday, January 14, by Secretary W. M. Jardine in the presence of Senators and Representatives in Congress and many of the members of the staff.

Secretary Jardine presided at the ceremony. In response to his invitation, Senator Capper and Representatives Haugen and Dickinson made brief talks,

all expressing their satisfaction at the prospect that the Department of Agriculture, now scattered throughout the District of Columbia in many buildings, is soon to have a unified and adequate housing for its business and activities in the National Capital.

Following the remarks by the members of Congress, Secretary Jardine delivered an address. He sketched briefly the history of the Department of Agriculture and the needs of the department for housing which the new construction will meet. Regarding the new building he said:

This central unit which connects the two wings will be ready for occupancy about May of next year, at a cost of $2,000,000. It will have a floor space of 73,000 square feet, and together with the two wings will give the completed building 229,000 square feet. This central unit will house only the administrative forces.

Fortunately, provision has been made for the construction also of the first unit of the extensible building south of B Street. This unit, which will cover an entire city block, will furnish an additional 320,000 square feet of space, and $5,750,000 has been authorized for the construction of the entire extensible building. It will later be extended by the addition of wings east and west over two more city blocks.

When this building program is completed we will have what promises to be adequate accommodation, at least for a considerable time. It will then no longer be necessary for the department to pay something like $200,000 a year for unsatisfactory accommodation, and then, for the first time in its history, the department will have a physical plant in Washington suited to the needs of its growing and vitally important work.

This building has been designed with a dignity and simplicity very appropriate for the national headquarters for work in agriculture. Twelve Corinthian columns of white marble will be a striking feature of the central unit, which, along with the two wings, will be built of white marble. The entire building will have a marble frontage of 750 feet. Its interior will be substantial and serviceable for offices, libraries and laboratories, but without elaborate or ornate finish.

SCIENTIFIC NOTES AND NEWS

Dr. Thomas Burr Osborne, since 1886 research chemist in the Connecticut Experiment Station, distinguished for research on the chemistry of the vegetable proteins and related subjects, died on January 29 in his seventieth year.

Professor A. A. Michelson, of the University of Chicago, and Dr. R. A. Millikan, of the California Institute of Technology, will receive the gold medals of the Society of Arts and Sciences at a dinner at the Biltmore Hotel, New York, on February 22. The medals have been awarded for distinguished service in science. Mr. Gerard Swope will be toastmaster and the speakers will include a number of scientific men and industrial leaders.

Dr. George D. Birkhoff, professor of mathematics in Harvard University, has been elected a corresponding member of the French Academy of Sciences.

Dr. Ludvig Hektoen, chairman of the department of pathology of the University of Chicago and director of the McCormick Institute for Infectious Diseases, was elected at the recent New York meeting president of the Society of American Bacteriologists.

Officers for the year 1929 have been elected by the Philosophical Society of Washington as follows: *President,* Leason H. Adams, Geophysical Laboratory; *corresponding secretary,* Edgar W. Woolard, George Washington University; *recording secretary,* Oscar S. Adams, Coast and Geodetic Survey; *treasurer,* O. H. Gish, Department of Terrestrial Magnetism.

Professor E. J. Stirniman, of the agricultural engineering division of the University of California, was named chairman of the Pacific Coast section of the American Society of Agricultural Engineers at its recent annual meeting. W. L. Paul, of San Francisco, was made first vice-president; G. P. Smith, University of Arizona, second vice-president; W. L. Power, Oregon State College, third vice-president, and W. W. Weir, University of California, secretary-treasurer.

The annual meeting of the American Institute of Mining and Metallurgical Engineers will be held from February 18 to 21, inclusive, and will include, among other features, an all-day symposium on corrosion. The meeting will close with a lecture by U. R. Evans, of Cambridge, England, on "The Passivity of Metals and its Relation to Problems of Corrosion." The sessions will be held in the auditorium of the American Institute of Mining and Metallurgical Engineers, 29 West 39th Street, New York City.

After thirty years of gratuitous service the original editorial board of *Rhodora,* journal of the New England Botanical Club, has asked to be relieved and the council of the club has appointed a new board to continue the journal. The new editors are Merritt Lyndon Fernald, editor-in-chief; James Franklin Collins, Charles Alfred Weatherby, Ludlow Griscom and Carroll William Dodge, associate editors. Manuscripts for consideration should be sent to M. L. Fernald, 14 Hawthorn Street; subscriptions to Ludlow Griscom, Museum of Comparative Zoology, Cambridge, Massachusetts.

Scott Turner, director of the U. S. Bureau of Mines, has been designated by Secretary of Commerce

Whiting to represent the Department of Commerce on the advisory committee of the Federal Oil Conservation Board.

Thomas W. Lamont, the banker, has accepted the chairmanship of the newly formed Diphtheria Prevention Commission of New York City, recently organized with inoculation clinics scattered over the city.

Dr. William J. Schieffelin was reelected head of the American Mission to Lepers at the annual meeting on January 14. It was decided to send Dr. Lee S. Huizenga on a year's tour of countries where leprosy prevails to attend conferences, to stress the need for action against leprosy and to initiate programs of public health education. He will inform local physicians, chiefly in Asia, of the modern treatment of leprosy and will urge the establishment of out-patient clinics.

Dr. Haven Emerson, professor of public health administration and director of the institute of public health of the College of Physicians and Surgeons, Columbia University, sailed on February 2 to make a survey of public health and sanitation in Greece, on the recommendation of the health section of the League of Nations.

Dr. C. W. Warburton, director of extension work, sailed for Porto Rico on January 10, to represent Secretary Jardine on the Porto Rico Hurricane Relief Commission designated to administer a fund of $6,000,000 authorized by Congress for the rehabilitation of Porto Rican agriculture. The commission consists of the Secretary of War, chairman; the Secretary of the Treasury, and the Secretary of Agriculture. The Secretary of War is being represented by Major C. S. Ridley, of the Engineer Corps, and the Secretary of the Treasury by A. G. Redpath, special assistant to the undersecretary of the Treasury. The representatives of the three departments will study together the conditions in the island and present a plan for the administration of the fund.

Professor W. L. Jepson, of the department of botany of the University of California, has recently returned from southern California, where he was called to advise on the establishment of a new botanic garden on the Rancho Santa Ana, near Anaheim. The new garden will be devoted entirely to native California plants.

Professor Oakes Ames, of Harvard University, chairman of the council of botanical collections and supervisor of the Biological Laboratory and Botanic Garden in Cuba, will visit the garden. He intends to carry on research work in connection with economic botany and to make a survey of the station with a view to obtaining data for possible alterations to be made in the future.

Professor Alfred S. Romer, curator of the Walker Museum of the University of Chicago, and Paul C. Miller, associate curator, leave about April 1 for Cape Town for a nine months' expedition to South Africa to study the structural evolution of mammals. Half the funds to finance the work have been provided by a citizen of Chicago who prefers to remain anonymous, the other half by the university.

Dr. T. Addis, professor of medicine in the Stanford University School of Medicine, is absent on leave for one year, acting as guest physician in the Hospital of the Rockefeller Institute for Medical Research.

Professor Linwood L. Lee, research specialist in land utilization for the New Jersey Agricultural Experiment Station, has been granted a year's leave of absence to go to the Rothamsted Agricultural Experiment Station, Harpenden, England, to make a comparative study of British and American methods of soil examination.

Dr. C. E. McClung, professor of zoology at the University of Pennsylvania, lectured on January 2[illegible] before the Swarthmore chapter of the Society of Sigma Xi on "The Material Basis of Heredity."

Dr. J. R. Magness, head of the department of horticulture at the State College of Washington, lectured on January 4 before the Purdue University Chapter of Sigma Xi on "Plant Respiration with special Reference to Fruit Storage."

Dr. S. R. Detwiler, professor of anatomy in the College of Physicians and Surgeons of Columbia University, lectured at Mount Holyoke College on January 18 on "The Growth of Nerves."

Dr. Kurt Koffka, professor of research in psychology at Smith College, will give four lectures on the "Gestalt" theory at the New School for Social Research, New York City, on Tuesday evenings at 8:30 P. M., beginning on February 12.

On January 19, Dr. Arthur L. Day, director of the Geophysical Laboratory of the Carnegie Institution of Washington, delivered an address before the Royal Canadian Institute, Toronto, on "Boring for Natural Steam Power in California."

The twenty-sixth lecture of the Rush Society of the University of Pennsylvania was given on January 2[illegible] by Colonel L. W. Harrison, D. S. O., adviser to the

British Ministry of Health on venereal diseases. His subject was "Principles and Results of the British Method of Venereal Disease Control." Colonel Harrison is lecturer on venereal diseases and director of the clinic at St. Thomas Hospital, London.

PROFESSOR GEORGE HERBERT MEAD, professor of philosophy in the University of Chicago, has been chosen Carus lecturer before the American Philosophical Association for the year 1929. The lectures, which have been previously given by Professor John Dewey, of Columbia University, and Professor Arthur Lovejoy, of the Johns Hopkins University, are given every three years at the joint meeting of all divisions of the American Philosophical Society, and are published by the Open Court Publishing Company of Chicago.

PROFESSOR JOHN DEWEY, of Columbia University, will leave for Scotland early in March to deliver a course of Gifford lectures in the University of Edinburgh. Dr. Dewey expects to be abroad for six months.

ROBERT LINCOLN SLAGLE, president since 1914 of the University of South Dakota and earlier professor of chemistry, died suddenly on January 31, aged sixty-three years.

CHARLES BINGHAM COCHRAN, since 1918 chemist of the Charles E. Hines Company, for twenty-three years dairy and food commissioner of the Pennsylvania State Department of Agriculture, died on January 22, in his seventy-fifth year.

WILLIAM LYMAN UNDERWOOD, since 1900 lecturer in the department of biology of the Massachusetts Institute of Technology, died on January 28.

DR. VICTOR HUGO JACKSON, emeritus professor of orthodontia at the University of Buffalo, who since 1915 has practiced in New York City, died on January 26 in his seventy-ninth year.

DR. ARTHUR EUGENE EWING, professor emeritus of ophthalmology in Washington University, has died at the age of seventy-three years.

THE death at the age of eighty-seven years is reported from Geneva of Dr. Jacques Reverdin, a former professor at the University of Geneva and one of the founders of the Geneva Faculty of Medicine.

THE staff of the Department of Tropical Medicine of the Harvard Medical College has passed the following resolution: "*Whereas,* Through the death of Dr. Frederick C. Shattuck, Jackson Professor of Medicine, emeritus, Harvard Medical School, the Department of Tropical Medicine has suffered the loss of its invaluable counsellor and supporter and friend; who during the past sixteen years has through the force of his wisdom, his interest and generosity made possible the development of both humanitarian work and scientific investigation in Tropical Medicine; one whose character and indefatigable industry and courage have often been an inspiration; who from his ability and ripe experience has furnished valuable suggestions on problems for study, and plans and means for their solution; We, the members of this Department of Tropical Medicine, desire especially here to record our appreciation of all that Dr. Shattuck has done in this respect, and to express our grief and sense of personal loss at the passing of so beloved a physician and true friend."

TRIBUTES were paid to the memory of the late Dr. Charles James, formerly head of the department of chemistry in the college of technology at the University of New Hampshire, at the weekly convocation exercises held in the gymnasium in January. President Edward M. Lewis presided and spoke on the loss of one of the leaders of the institution who was a member of the faculty for nearly a quarter of a century. Dr. Hermon L. Slobin, director of the Graduate School, reviewed briefly Dr. James's life and accomplishments. The chemistry building now under construction at the west corner of the campus will be named Charles James Hall.

A BOTANICAL collection of more than 3,000 specimens has been presented to the University of California by Dr. A. W. Hill, director of the Royal Botanic Garden, Kew, England. Dr. Hill lectured at the University of California in 1926.

UNIVERSITY AND EDUCATIONAL NOTES

THE School of Nursing of Yale University has received a gift of $1,000,000 for endowment from the Rockefeller Foundation.

A GIFT of $100,000 has been made by Dr. and Mrs. W. Wilbur Beckett to the endowment fund of the school of medicine of the University of Southern California.

THE will of Henry C. Munger, of Plainfield, N. J., provides charitable bequests of $960,000. Mount Holyoke receives $200,000.

ARRANGEMENTS have been completed whereby the department of zoology at the University of Texas will have $135,000 to be used in the development of graduate instruction and research during the next eight

years. Approximately one half of this sum will be given by the General Education Board; the rest will be raised by the university. The zoological staff plans to use a portion of this fund for technical assistance and for fellowships.

As reported in SCIENCE, a citizen has given to the University of Sydney, Australia, about $1,000,000. The *Journal* of the American Medical Association states that this gift is "for the specific purpose of establishing three chairs in the medical school and of equipping the necessary laboratories." According to the *Medical Journal of Australia,* the new professors of medicine, surgery and bacteriology will be required to devote their whole time to their tasks, which will include research. Heretofore the chairs of medicine and surgery have been filled by part-time professors. The salaries of the new professors of medicine and surgery are understood to be £3,000.

DR. PAUL S. MCKIBBEN, professor of anatomy at the University of Michigan, has been appointed to the chair of anatomy in the school of medicine of the University of Southern California, and Dr. Harry J. Deuel, professor of physiology at the University of Maryland, to the professorship in biochemistry.

DR. CHAS. GURCHOT and Miss Frances Watson have been appointed research assistants in the department of pharmacology and the division of neurology of Stanford University School of Medicine, assisting in researches on syphilis supported by a grant from the Committee on Research in Syphilis to Dr. P. J. Hanzlik and Dr. H. G. Mehrtens.

R. KEITH CANNAN, senior lecturer in biochemistry at University College, London, will give two courses this summer at the school of medicine of Western Reserve University under the auspices of the summer session. In addition to conducting a brief course in biochemistry, he will give a series of lectures upon "Biological Oxidations and Reductions."

SPECIAL CORRESPONDENCE

THE GENEVA SUMMER SCHOOL OF ALPINE GEOLOGY[1]

LED by Professor Léon W. Collet, professor of geology at Geneva and Harvard universities, with the collaboration of his chief assistant, Dr. Ed. Paréjas, a group of British and American geologists, including Professor and Mrs. Kirtley Mather, of Harvard University, and their party of eleven students, spent a memorable three weeks during July of the past summer, in the study of Alpine geology in the field, going on foot with rucksacks through the high mountains and traveling along the valleys by motor and train. The course included a study of the Alpine foreland, which comprises the Jura Mountains, the Swiss Plateau, the High Calcareous Alps (the Morcles, Diablerets and Wildhorn Nappes) and the crystalline massifs of the Aiguilles Rouges, Mont Blanc, Gastern and Aar. The latter part of the three weeks was spent in a study of the geosyncline south of the foreland, which is represented by the Simplon, Saint Bernard, Monte Rosa and Dent Blanche Nappes. The hinterland, which was not studied in the field, forms the southern boundary of the Alps. It is the southern jaw of the vice, which has approached the northern one (the foreland) to produce the intense folding and thrusting of Alpine structures. For the follower of Wegener, the hinterland is, of course, Africa or Gondwanaland. (A portion of the hinterland which was thrust over the geosyncline and the foreland is represented in the west by the Prealps and in the east by the Austrian Alps.)

Starting from Geneva the party first crossed the Swiss Plateau where the Molasse, a thick series of Tertiary sandstones and conglomerates, was deposited at the northern foot of the then still growing Alps, whose advancing nappes finally overrode the southern edge of their own débris. Forming the northern rim of the plateau are the Jura Mountains, which lie in a great arc to the northwest of the Alps. They are, indeed, but a branch or virgation from the bow of the western Alps, having been formed by the northward push of the nappes. Here the party saw the steeply folded, underthrust, asymmetrical, pitching and faulted anticlines so characteristic of Jura structure. These structures become even more interesting in the light of Buxtorf's discovery that the crystalline rock of the substratum, the Permian and even the oldest strata of the Trias have remained undeformed beneath the crumpled structures of the Jura. These folds of the Jura have been developed upon the lubricating salt-bearing shale beds of the middle Muschelkalk which lie at the base of the folds. The deformed beds have thus suffered a *décollement* over the basement rocks.

Other features of great interest in the Jura were the tear faults, approximately vertical zones of more or less nearly horizontal movement, cutting across the strike of the folds and diverging to the north.

[1] In this very brief résumé, no effort has been made to cite references. For an excellent account of the geology of the Alps the reader is referred to Professor Collet's book, "The Structure of the Alps" (London, Edward Arnold Co., 1927), which contains also complete bibliographies.

By this displacement, whose effect on the topography is often striking, an elongation of ten kilometers in the arc of the folded Jura has been produced.

Leaving the Jura and passing southward from Geneva to cross the Prealps and the ranges of the High Calcareous Alps, the party made the ascent from Chamonix to Montenvers, in the Mont Blanc massif, that magnificent and ancient range which dominates the scenery of the western Alps. Long before the birth of the Alps, it had passed through the throes of Hercynian mountain building whose record it still carries in the north-south orientation of its schists and of the inclusions and phenocrysts of its granite. Standing submerged in the early Mesozoic at the northern border of the Alpine geosyncline, it received a cover of shallow water limestone of Triassic and Jurassic age. Forming a part of the foreland, which was the northern jaw of the vice in which the sediments of the geosyncline were compressed to become the nappes[2] or recumbent anticlines of the Pennine Alps, the Mont Blanc massif was caught in the convulsive northward movement of the Oligocene. At this time the entire crystalline massif was thrust toward the north, splintering into a multitude of wedges and slices while its sedimentary cover was thrown into a series of northward-traveling folds. The massif was itself overridden by the Pennine Nappes, which rose from the geosyncline to the south, and was finally reexhumed by erosion to occupy the dominating position which it holds to-day.

Crossing again the zone of Chamonix, which separates the Mont Blanc from the Aiguilles Rouges, the party mounted to the plain of Salenfe, the floor of an enormous glacial cirque eaten into the heart of the Aiguilles Rouges massif. Here the granites and schists of the massif itself are overlain by the Trias of the autochthon or original sedimentary cover. Lying above this and separated from it by a thrust plane is a great recumbent anticline with Tertiary rocks at the base of the reversed limb, Middle Jurassic at the core and Upper Jurassic at the top. It is a part of the frontal fold of the Morcles Nappe which has traveled northwestward from the zone of Chamonix along the clean-cut thrust at its base. Into the core of this nappe the upper portion of the cirque of Salenfe has been carved, exposing the grandest features as well as the most minute details of its structure in the precipitous slopes of the Tour Saillere, which stands out in the morning sunlight like an enormous text-book diagram. Along the thrust plane of the nappe, lying between the nappe and the autochthon, one may observe in places a discontinuous layer of mylonite, the remnants of masses of gneiss which were torn from the crystalline basement by the moving nappe, and completely crushed and granulated as they were dragged along beneath it.

From Salenfe the party returned to the zone of Chamonix, and thence traveled eastward along the strike of the Morcles Nappe. They saw it pitching beneath the Diablerets and Wildhorn Nappes, higher elements of the High Calcareous Alps, and reappear with the axis elevation to the eastward, rising over the Gastern Massif, which corresponds structurally to that of the Aiguilles Rouges farther west.

From the Torrentalp above Loèche-les-Bains the party climbed to the Nieven pass in the zone of the Lötschenthal, which represents the eastward prolongation of the zone of Chamonix. On the way to the pass they saw, rising against the sky in four huge saw-toothed ridges, the four crystalline wedges whose upward and northward drive into the overlying limestones and shales had started the Morcles Nappe on its journey over the Gastern Massif, developing in the nappe the four recumbent frontal folds which are exposed in a single spectacular cliff immediately north of the wedges. Lying in the passes between the crystalline ridges are the remnants of the overturned synclines which lay between the forward-driving masses of crystallines. To see here the zone of the roots of the nappe where the crystalline wedges stand in such clear relationship to the sedimentaries of the synclines between them and to the recumbent folds of the nappe in front of them, was to feel oneself a witness to the very building of the Alps, so plainly are the tectonic features exposed in the scenery.

From the Lötschenthal the party climbed northward over the Gastern Massif and descended by the Tschingel Glacier to Lauterbrunnen at the foot of the Jungfrau mass. From Lauterbrunnen to the Rottal Hut of the Swiss Alpine Club the trail led over the foot of the limestone slopes of the Jungfrau group, which rests on the crystalline rocks of the Gastern Massif. At the base of the limestone series are the yellow, the red and the black beds of the gently dipping Triassic, followed by the gray and the brownish rocks of the Jurassic. This is the normal series of the autochthon, resting on the crystallines with a sedimentary contact. Climbing higher, one again perceived yellow limestones of the Triassic, and above them beds of the Jurassic, which belong to a second normal series exactly like the first and rest upon it with a mechanical contact. This duplication within

[2] A true nappe is a recumbent anticline whose reversed limb has partly disappeared owing to stretching. The term is applied also to recumbent anticlines such as the Pennine Nappes.

the autochthon is a common feature, caused by the northward thrusting of the overriding nappes and wedges which dragged slices of the autochthon beneath them and piled them up where local conditions permitted.

Penetrating into these piled-up limestones is an enormous wedge of crystallines originating from the underlying Gastern Massif. It is covered by a layer of limestone and by a zone of gneiss, intensely crushed and mylonitized, which wraps around it. Above the autochthon may be seen the gray wall of the Morcles Nappe, a quite different tectonical element which has come from the south. In this, the Cretaceous, lacking in the lower series, is present, denoting that sedimentation in the site of deposition to the south was different from that which occurred on the Gastern Massif, the place of origin of the autochthon.

At the summit of the Jungfrau mass, the granites which have been thrust up over the nappe from the south are revealed in the ruddy hues of the peaks. Here again one sees with startling clarity the structure of the mountains; the Gastern crystallines of the basement of the Jungfrau; their original sedimentary cover, sliced and stacked up in repeated series; driven into these a huge crystalline wedge, and driven over them the sedimentaries of the Morcles Nappe; and capping it all, that amazing mass of granite which has ridden up over the nappe itself to form the summits of the range.

Striding over the ground covered by the party in the trip from Lauterbrunnen to Zermatt in the region of the Pennine Nappes of the geosyncline, we leave the massifs of the foreland and the Morcles Nappe, itself a part of the foreland, and pass directly to the geology of the geosyncline, that portion of the ancient sea of Tethys which was the birthplace of the Alps. Among the most interesting of the differences between the rocks of the High Calcareous Alps of the foreland and those of the Pennine Nappes of the geosyncline is in the character of the sediments. While those of the foreland are predominantly limestones of shallow water and reef types, those of the geosyncline represent a deep-water facies. Indeed, radiolarites associated with manganese ores occurring in the youngest of these deposits, the *Schistes lustrés*,[3] are so similar to certain deep-sea deposits of the present day that they are supposed by Alpine geologists to represent a true abyssal facies. In equally striking contrast to the nature of the rocks of the Morcles Nappe is the relatively high grade of metamorphism which characterizes the material involved in the folding of the Pennine Nappes. And yet ammonites have been found in these rocks, even in garnetiferous zones of the *Schistes lustrés*.

From the Gornergrat, above Zermatt, the geologist sees a magnificent panorama of the uppermost of the Pennine Nappes: the Saint Bernard, the Monte Rosa and the Dent Blanche. Two of these, the Saint Bernard and the Dent Blanche, have existed since Carboniferous time as geanticlines which rose, early in their history, above the level of the geosynclinal sea. About the shores of these embryonic nappes coarse breccias and conglomerates were deposited while the *Schistes lustrés* were being laid down in the deep water of the synclines. The development of these nappes was, then, only the culminating event of a history which was amazingly long, even in terms of geological chronology. Throughout Mesozoic times the embryonic nappes continued to grow, becoming more and more strongly overturned, until finally, in the paroxysmic folding of mid-Oligocene times, they started on their northward journey and became the dominant elements in the structure of the geosyncline. Thus the lower nappes of the geosyncline, the Simplon assemblage, were set in motion by the Saint Bernard, and the Monte Rosa group by the Dent Blanche Nappe.

At the base of the Dent Blanche Nappe, in the Unter Gabelhorn, was observed one of the most interesting of the details of Alpine geology. Here a sill of gabbro eighty to one hundred meters in thickness has been intruded along the thrust plane and suffered terrific crushing under the moving nappe. One is immediately reminded of the "Sima" of Wegener's hypothesis.

From the Gornergrat, above Zermatt, the observer also has a splendid view of the remnants of an ancient mature valley six to seven kilometers wide, whose uplift, amounting to about twenty-five hundred meters, represents the most recent of major Alpine movements, having occurred at the beginning of the Quaternary.

At Zermatt the course ended, leaving its participants breathless at the magnitude, not only of Alpine structures and the movements which they represent, but also at the colossal piece of physical and intellectual work which has been done by the geologists who have unraveled the story. And no less breathtaking is the magnificence and completeness of the exposures, and the certainty with which major structures may be worked out in three dimensions, once the stratigraphy is grasped. One never need dig in rabbit-holes in the Alps!

RUTH ALLEN DOGGETT

RADCLIFFE COLLEGE

[3] The French word *schiste* is, like the German *Schiefer*, applied to any rock possessing marked fissility and does not necessarily denote one of metamorphic origin.

DISCUSSION

SCIENCE, METAPHYSICS AND BLOOD

HAVING been assigned by the editor the pleasant duty of reviewing for SCIENCE Professor Lawrence J. Henderson's new book, embodying his Silliman Lectures on "Blood. A Study in General Physiology,"[1] and being just on the point of starting to write the review when SCIENCE of January 11, 1929, reached my desk, I read with great interest Professor Yandell Henderson's article in that issue entitled "Is this Science or Metaphysics?" At the bottom of page 39 I concluded that my job had been done for me, and that for once Providence, this time oddly disguised as a Yale professor, had subtracted from the obligations of an already overburdened life, rather than, as usual, adding to them. As I read on through pages 40 and 41, however, it seemed apparent that this was a too hasty conclusion. Something now needed to be said about Yandell[2] as well as about Lawrence.[2] Life almost always turns out that way.

Let me begin by stating that what follows is in no sense to be regarded as a defense of the book under discussion against the attack which has been made upon it. Many years ago there was revealed to me the simple but profound truth that the final and conclusive evaluation of all scientific work is determined by the intrinsic merits or demerits of the work itself, and not at all by what *anybody* says about it, either for or against. There appears no present reason to suppose that this law of human biology will not operate in this instance.

The purposes of this note are, first, to discuss some aspects of the book which, as it seems to me, were overlooked or neglected in the article referred to; and, second, to present another point of view regarding some general questions of scientific methodology which were raised in that article. "Blood" seems to me to be a more important book than it does to Professor Yandell Henderson, and this for three reasons. The first is that the opening chapter of the book impresses me as, on the whole, the most sound, penetrating and illuminating statement that has yet been made of the present status and the ideational development of biology as a science, on the one hand, and of the essential problem of that science—its basic *Fragestellung*—on the other hand. In the passage of time this chapter will come to be regarded a classic of biological literature, unless my judgment is greatly at fault, quite apart from any consideration as to what the ultimate evaluation of the rest of the book may prove to be.

[1] Yale University Press, 1928.

[2] Henderson.

The second reason why the book seems to me important is because of the methodology of its major portion, which deals with the experimental details regarding the physiology of the blood. The essential point of this methodology is what may be characterized as the *multiple free variable* experimental technique. It has long been a working canon of investigation in biology that what the experimenter should endeavor to do is to keep all possible *other* variables, internal and external, constant while he observed what happened relative to just *one*. In actual fact, owing primarily to the enormous intercorrelated complexity of the living organism, as well as to the extraordinary practical difficulty of keeping constant even the most simple and basic of the elements in the purely physical environment, this ideal is practically impossible of achievement experimentally, if one is thinking or working in terms of quantitative precision of anything like the same order as those in which the physicist or chemist works. Furthermore, experience in the other sciences, particularly physics, has demonstrated that it is a relatively sterile and unfruitful methodological ideal at best. Nature in general and organisms in particular, are *organized*. Event A is one thing when B_1, C_1, D_1 . . . N_1 are happening, and quite another when B_2, C_2, D_2 . . . N_2 are happening, B, C, D, etc., being events which vary as indicated by the subscripts. As Whitehead[3] says, even "an electron within a living body is different from an electron outside it, by reason of the plan of the body." Now the traditional methodological canon says that the best thing to do in making experiments is first to keep B_1, C_1, D_1 . . . N_1 constant and see what A looks like; then to hold C_1, D_1, . . . N_1 constant, and let B change from B_1 to B_2, and see what happens to A. But above all, keep everything else possible in the system except A and B under constraint while the investigation of their interrelationship is in hand. What this methodological scheme deliberately neglects (though of course no intelligent investigator overlooks it) is that when C, D, . . . N are put under constraint A *is also and by virtue of that fact put under constraint*. And it is the behavior of A over its whole range of possible behavior that, by hypothesis, we want to find out about.

There is perhaps no better example to be found of the relative sterility of this methodology in biology as compared with the fruitfulness of the multiple free variable technique than is afforded by the history of Mendelism. Before Bateson and Punnett in England and Morgan in this country got started upon the investigation of dihybrid and trihybrid ratios, out of which came the whole present-day conception of the relations of genes and the mechanism of heredity,

[3] A. N. Whitehead, "Science and the Modern World," p. 111. 1925.

Mendelism was already being said to have exhausted itself. All that was exhausted was a single constrained variable methodology.

The essential point in the methodology of Lawrence Henderson's experimental study of the physiology of the blood is that he has, in effect, observed and quantitatively measured *simultaneously,* at suitably separated intervals of time and space, the state at that instant of a whole series of *freely varying* physical, chemical, physicochemical and physiological elementary variables connected with the blood. The result of this technique has been to reach a wholly different order of understanding of the physiology of the regulation of the internal environment of the organism (to use Claude Bernard's phrase) than we have ever had before. It is not intended to imply that Lawrence Henderson is the first or the only investigator[4] who has applied this methodological technique in biology. What he has done, however, is to use it more consistently, intelligently and effectively for the solution of a definite, particular problem than has any one hitherto, so far as I am aware.

The third respect in which the book seems to me to be important is in that it achieves, within modest but by comparison considerable limits, a real *synthesis* of a previously scattered and only very partially integrated set of biological observations. Books which even attempt true synthesis are extremely rare in biology; those that achieve it in any degree are still rarer.

So much for my opinions about the book; now just a few words about Professor Yandell Henderson's. He appears to object to it on three main grounds: he doesn't like the style in which it is written; he thinks it is metaphysics and not science, and finally he thinks that its author has not been sufficiently polite to other workers on the physiology of the blood. Regarding the first of these points there seems little chance of doing anything. It is probable almost to certainty that Professor Lawrence Henderson will not change his manner of expression. In fact it is inconceivable. Nor, on the other hand, can any of Professor Yandell Henderson's friends suppose that he is going to alter his literary tastes. In emotional matters he is known not to be an altering kind of person. Plainly there is no hope on this first point. There is, however, one specific matter here to which exception must be taken. A few sentences are quoted and it is said that this statement might have emanated from Paracelsus. But has Professor Yandell Henderson ever read Paracelsus, or has he just read about him? For surely two men were never further apart in literary style than these. That the readers of SCIENCE may judge the point for themselves let me quote a few sentences from the introduction to an English translation of Paracelsus' "Nine Books on the Nature of Things," published in 1650, which happens to be on my table as I write. He is speaking of sophisters and scoffers

> who contemn all things, which are not agreeable to them, and indeed detract from them: These are pleased onely with what is their own, as indeed all fooles are wont to be, whom their owne toyes onely please, not anything which is anothers, hating all kinds of wisdome. Wherefore they account wisdom as folly: because nothing doth them any good they know the use of nothing. As one workman cannot use the instruments of another, so a foole can use no weapons better than his owne sticks, or boughes; and there is no sound pleasanter to him than the ringing of his own bell.

Now would any one maintain that Lawrence J. Henderson ever did, or could, or would, write like that?

Regarding the metaphysical disability I confess myself to be in doubt, because nowhere does Professor Yandell Henderson define precisely what he understands by metaphysics. He just calls it "that most insidious disease of scientific thought." But, as he complains about the book, this is not informative. From the context of the article as a whole, one reader, at least, gets the impression that he regards everything in science which is not the purest naive empiricism as metaphysics. But Professor E. A. Burtt,[5] who certainly has a clear notion of what metaphysics is, says (p. 137): "Of course Newton's conscious reaction to metaphysics was one of vigorous opposition, as to a collection of quite unverifiable 'hypotheses,' but since no one can avoid ultimate assumptions of some sort he was, like most scientists, a metaphysician against his will." Perhaps, in this respect, Professor Yandell Henderson is like Newton. But again perhaps he isn't. There seems to be no way to resolve this second difficulty until he defines his conception of metaphysics more precisely than as an "insidious disease."

Regarding the third point of objection to the book, my statistical instincts come to the fore and suggest an objective inquiry. With the help of the excellent index, plus a patient reading of pages where the lazy *"ff"* makes it necessary, the following table has been constructed, showing the number of different pages on which the leading investigators of the physiology of

[4] In a very modest way the present writer has endeavored to use this methodology in his investigations on duration of life described in "The Rate of Living." New York (Knopf). 1928.

[5] E. A. Burtt, "The Contemporary Significance of Newton's Metaphysics," in "Isaac Newton, 1642–1727. A Memorial Volume." London (G. Bell and Sons, Ltd.). 1927.

the blood (other than Professor Lawrence Henderson's students and associates) are specifically mentioned or their work is discussed or both. The names are arranged in descending order of frequency of mention.

Name	Number of different pages on which investigator and his work are named or discussed
Bernard	8
Haldane	8
Krogh	5
Barcroft	4
Hasselbalch	4
Bohr	3
Yandell Henderson	2

Surely the facts disclosed by this table give no ground for the grievance that predecessors and contemporaries do not receive adequate recognition. Or do they?

RAYMOND PEARL

INSTITUTE FOR BIOLOGICAL RESEARCH,
JOHNS HOPKINS UNIVERSITY

THE 1928 SILLIMAN LECTURES

THE last Silliman Lectures at Yale were delivered by Professor L. J. Henderson on a field of physiology to which he has devoted himself during the past twenty years, *viz.*, the relations between the different electrolytes, gases and proteins in the blood, and the alterations in those relations that occur during normal and pathological metabolism. The publication of these lectures in book form has drawn from Professor Yandell Henderson the savage criticism which appeared in SCIENCE of January 11. Independent opinions concerning the relative value of the studies presented and of the criticism against them can be formed only by the few who are themselves engaged in the intricate field of research covered. Hence it appears that, in fairness to those readers of SCIENCE who lack the concrete knowledge, Yandell Henderson's remarks should be reviewed by another student in the field who has formed quite a different opinion.

Essentially Yandell Henderson's criticisms may be condensed to two: (1) that Lawrence Henderson has failed to give due credit to Haldane's magnificent work, and (2) that the lectures are metaphysical.

The first criticism can be met by any one who refers to the several places where Haldane's work is mentioned in the lectures. In the writer's opinion there is no basis for complaint. The lectures are in their nature a review of Lawrence Henderson's personal work, and where it is based upon Haldane's previous discoveries that fact is acknowledged. Yandell Henderson, as an example of insufficient appreciation, quotes a paragraph from the lectures which ends with the statement, "This conclusion escaped us all, and it remained for Christiansen, Douglas and Haldane to discover by experiment that the carbon dioxide dissociation curves of oxygenatized and reduced blood are different." This statement is, it appears to the writer, a sportsmanlike acknowledgement of a debt due Haldane and his collaborators for solution of a problem which, despite its outstanding importance, had eluded other investigators.

The charge of being metaphysical appears absurd against a work which contains 225 diagrams and 86 tables, presenting chiefly quantitative experimental results obtained in Lawrence Henderson's laboratory, together with an appendix on laboratory technique. The lectures, aside from their value in affording mathematical approaches to hitherto insoluble relationships, constitute a most useful compendium of concrete facts and figures to any worker in the field: so much so that the copy in our laboratory is seldom in its place on the shelf. In the introduction, it is true, Lawrence Henderson presents a view-point concerning the historical development of general biology and concerning modes of attack on its problems; and the concluding chapter is of a broadly reflective nature: both, to the writer, afford stimulating and profitable reading. In between are eleven chapters packed with concrete quantitative observations and calculations based upon them.

DONALD D. VAN SLYKE

ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH,
NEW YORK, N. Y.

THE APPORTIONMENT SITUATION IN CONGRESS

THE apportionment problem will probably be considered again by the House of Representatives during the present session of Congress. Because of that fact and because my attitude towards it is not adequately stated in Professor Huntington's article in SCIENCE for December 14 (pages 579–582), I am glad to outline briefly the situation as I see it.

Neither the bill defeated last May nor the similar bill introduced at this session is a real apportionment bill. It is a bill authorizing a future apportionment by the secretary of commerce after the results of the census of 1930 or of any subsequent census have been announced and Congress has failed to pass an apportionment bill in the following session. Thus, if the field work on the next census should start in November, 1929, the population of the several states would doubtless be announced before Congress assembled in December, 1930. If no bill on apportionment should

become a law before the end of that session and if in the interim the bill defeated last May or some similar bill should have been passed, it would authorize and instruct the secretary of commerce to apportion the present number of representatives among the several states by the method used in the most recent apportionment, that of 1911, and report the results to Congress. These results would go into effect unless or until Congress changed them.

The advantage of the plan is that it would automatically readjust the existing number of representatives to each decennial shift of population, in case, and only in case, Congress failed to agree upon some other plan.

Upon the moot question of method, the main problems before the census committee which has reported the bill seem to have been these two:

(1) What method is likely to give the bill the best chance of passing Congress? The committee had to choose between a method which had been used in a previous apportionment and a novel, untried method. They selected the method of major fractions which had been used in 1911, believing, I suppose, that this choice would improve the bill's chance of passage. They are in a better position than an outsider to decide what method would be preferred by Congress, and the fact that, after considerable discussion, they introduced into this winter's bill the same method as that specified in the bill of last spring shows that in their judgment this method was not "a distinct hindrance to the passage of the bill."

(2) What method is most likely to satisfy Congress when its results are brought home to the members by a specific apportionment? At this point the method of major fractions has a marked advantage. If the secretary of commerce is called upon, for example, in 1930 to apportion 435 members by the method of major fractions, he would probably send to Congress not merely a list showing the number of representatives allotted to each state, but with it a table showing the population of each state by the latest census divided by a constant divisor and one representative allotted for each unit and each fraction larger than one half in the series of quotients. The whole series would sum to 435. But if the secretary of commerce had to perform the same apportionment by the method of equal proportions, he would probably report merely the number of representatives assigned to each state. If Congress then asked how his results were reached, he would reply either that the prescribed method did not lead up to a table such as Congress has had before it on every previous occasion, or by submitting a table with a constant divisor and a series of quotients, wherein a state with a quotient, for example, of above 1.414 and below 1.500, as well state with a quotient of above 1.500 and below 2. would receive two representatives, because 1.414 is geometric mean between 1 and 2, and 2.449 the metric mean between 2 and 3. In my judgment, C gress would not be satisfied with either form of re For this reason I believe that if the method of eq proportions should be substituted in the pending for the method of major fractions, the resulting would be more likely to go the way of the law of 1 which prescribed a method since recognized both special students and by Congress as unsatisfact Partly for that reason the law quickly became a d letter. I am anxious to avoid a similar failure of present effort.

The choice of a method seems to me of little portance compared with the need of securing cong sional compliance with the constitution. I wo gladly abandon my preference for the method major fractions if I thought another method ha better chance of acceptance by Congress and country. As many persons interested in the practi problem seem to be baffled by the mathematics of a portionment, let me state again, as simply as possi and without argument, the essential differences tween the different methods, taking for my exam the population of the states in 1920, but neglecti for the sake of simplicity, the constitutional guara to each state of at least one representative. The p cedure may be described as follows:

(1) Arrange the states in the order of decreasi population from New York to Nevada.

(2) Divide the population of each state by a figu slightly above the population of New York. The r sult would be a series of decimals ranging from . in the case of New York to .007 in that of Nevad

(3) Divide the population of each state by a figu slightly above that of Pennsylvania. The result wo be a series of quotients ranging from 1.198 in t case of New York and .999 in that of Pennsylvan down to .009 in that of Nevada, the quotient for ea state being larger than in the preceding division b by an amount which diminished with the total pop lation of the state involved.

(4) Continue this process, steadily decreasing t divisor and thus enlarging each quotient, until t resulting quotients give a House of Representati as large as desired.

The differences between the various methods of a portionment hinge upon the differences in weig given to the decimal fractions in these computatio

(1) At one extreme is the method of rejected fra tions used in all apportionments before 1840. By th method every decimal fraction, no matter how larg

disregarded. Thus, the division described in (2) uld give no representatives; that in (3) would give e to New York, etc.

(2) At the other extreme is the method of the harnic mean, or as I have preferred to call it in guing before the census committee, the method of nimum range. By it every decimal fraction, no tter how small, entitles the state to a representae. Thus, the division described in (2) would give rty-eight representatives; that in (3) would give rty-nine, etc.

(3) Next to the method of rejected fractions is e method of major fractions. By it every fraction rger than one half entitles the state to an additional presentative. Thus, the division described in (2) ould give four representatives to the four most pulous states; that described in (3) would give five, c.

(4) Between this and the method of the harmonic ean is the method of equal proportions. By it ery quotient above the geometric mean between the o numbers of representatives under consideration titles the state to the larger number. Thus, the vision described in (2) would give forty-eight reprentatives; that described in (3) would also give fortyght.

During the many years that I have worked upon e problem of federal apportionment, my main obct has been to improve upon the method apparently referred by Congress. Many scholars at various mes have suggested methods which they thought etter; Congress has rejected them all. The only evolutionary change of method ever made resulted rom the constitutional argument of Daniel Webster hen chairman of a Senate committee on apportionent. The report of his committee argued that every emainder above one half entitled a state to an addiional member. The Vinton method adopted in 1850 as supposed at that time to be merely a variant of Webster's method. My contribution has made Webter's method more workable.

From the point of view of Congress and the average itizen I would arrange the methods in the order of ecreasing persuasiveness, as follows:

Method of major fractions
Method of minimum range
Method of rejected fractions
Method of equal proportions

On scientific grounds I would place them in the ame order, if we take as a criterion, as I think we hould, the degree to which the several methods satisfy he legitimate purposes of the constitution and of Congress.

The main object which Congress and the country desire to realize by an apportionment is in my opinion either one of these two:

(1) To give the residents of the United States as nearly as may be equal representation in the House of Representatives, irrespective of the state of residence; or

(2) To give the members of the House of Representatives as nearly as may be equal numbers of constituents.

It might seem as if these two objects were one and the same, although viewed from different sides. But in fact they lead to different methods of apportionment. If the first is the controlling object, the method of major fractions is the one to be used. If the second is the controlling object, the method of minimum range is the one to be used. If the two are to be given equal weight, or an average is to be struck between them, the method of equal proportions is the one to be used.

The preceding statement probably reveals my reasons for thinking it undesirable "to request a report on the mathematical facts from the National Academy of Sciences." The fundamental problems are political. What is the main object of apportionment? What method of apportionment is best calculated to satisfy Congress and the country? On problems of this sort the judgment of the average representative or congressional committee is of far more importance than that of any group of scholars.

WALTER F. WILLCOX

CORNELL UNIVERSITY,
DECEMBER 24, 1928

"UNPROFITABLE METEORS" PAY LARGE DIVIDENDS

IN the December 14, 1928, issue of SCIENCE, pages 590–1, there appears an article by my good friend Dr. Heber D. Curtis, director of the Allegheny Observatory, entitled "Unprofitable Meteors." Apparently its publication was caused by annoyance and loss of time sustained by him due to people desiring further information about the Perseid and Leonid meteor showers of this year. As a result he is rather hard on the newspaper reporters for sensational articles on the subject, and indirectly even harder on professional astronomers who were obviously the sources of their information.

Nearly thirty years' acquaintance with Dr. Curtis, and a year or more of work as his assistant at Lick Observatory, have given me the highest opinion of him both as a man and a scientist. Paradoxically, it is for this very reason that I feel compelled to point out the true state of the case, in the same journal in which his note appeared, for otherwise I fear his remarks will do real harm to amateur astronomy.

Being actively in charge of the work of the American Meteor Society, most of the observations of meteors by amateurs in America go through my hands, and therefore I am in better position than most others to judge of its amount and value. Due to official connections with the International Astronomical Union, I am kept more or less informed of similar work done by amateurs abroad. This being the case, I can affirm that were it not for the work of the amateur, meteoric astronomy on the observational side would come almost to a standstill. This is especially true of America. As to the newspaper publicity, it is quite true that the writer did not give out personal articles: he does not know about the one "from Cambridge." But he was certainly responsible for getting Science Service to try to arouse the interest of amateurs. If the articles grew somewhat as various reporters "edited" them to suit home consumption, as they saw it, no great harm was done, as is proved by the fruits of the campaign.

To speak only of the Leonids, thanks very largely to this publicity, to date good reports have come (and they are still coming, as one arrived to-day) from New Zealand, Alabama, California, Kansas, Oklahoma, Pennsylvania, Texas, and Wisconsin, and poorer ones from other states. Several observers have also joined the A. M. S. who never knew of it before. Thanks to the aid of the U. S. Weather Bureau and the Hydrographic Office, U. S. N., also through publicity, Leonid fireballs have been reported from several ships at sea.

Briefly, the writer will eventually be able to publish from this data: Proof that the Leonid stream is wider than before thought as undoubted Leonids were seen from November 10 to November 19 inclusive; a good idea of the hourly rate and consequent density of the stream; good radiants on several dates; heights of a number of Leonids from duplicate observations in Texas; and the obvious result that this year's shower furnished unexpected numbers of fine fireballs and was twice as good as we expected. This increases our hopes for great showers between 1932 and 1934. But careful observations should be made from many stations every intervening year to give the best possible idea of what to prepare for as the time of maximum draws near. The Perseids also gave a good shower in August, as is usual, but lack of space forbids further mention of them here.

The writer therefore affirms that the results of the newspaper campaign, even with its obvious faults, more than justify it. As to people seeing no Leonids this November, there were just two reasons and no more: either they were not out in the cold observing for them or they did not have a clear sky. For many Leonids, and some very beautiful ones, certainly were seen from all stations where a proper watch was k[ept] and where the sky was favorable.

CHAS. P. OLIVIER

FLOWER OBSERVATORY,
UNIVERSITY OF PENNSYLVANIA,
DECEMBER 18, 1928

TERMINOLOGY OF "VITAMIN B"

THOSE who make a study of nutrition have no d[oubt] been interested and pleased to see that from [the] British side an attempt has been made[1] to re[ach] unanimity in the much-needed revision of the term[i]nology of the so-called "vitamin B." It has been su[g]gested that we shall continue to use the letter B [to] designate the "complex," B_1 for the antineuritic, le[ss] heat-stable factor, and B_2 for the more heat-stab[le] factor—the vitamin that ensures a normal increa[se] of bodily weight, stimulates appetite and has probab[ly] still other functions.

It stands to reason that the letter B does not suffi[ce] now that it has been proved that there are tw[o,] probably more, factors which differ considerably i[n] physiological action and in stability.

Meanwhile in the United States objections have bee[n] raised to the British proposal which make it appe[ar] doubtful that this terminology will be accepted. I[t] is not considered logical to go on using the old lette[r] B—even when accompanied by a figure for near[er] indication—for substances which are only in so f[ar] related as to occur together in certain vegetab[le] products, for instance in brewers' yeast, and can b[e] liberated from them by the same means of extraction.

Unfortunately no agreement has been reached among the American workers themselves. Sherman, to avoid any suggestion of connection, wants to d[o] away with the letter B, and in accordance with th[e] custom to indicate the vitamins alphabetically in th[e] order of their discovery, proposes the letters F and G—F to designate the less heat-stable factor and G t[o] identify the more heat-stable, growth-promoting one.

McCollum, who with Davis some years ago chose the letter B to refer to the antineuritic factor, wishe[s] to keep it for this, and suggests the letter F—or G—for the more heat-stable factor. Steenbock concur[s] with McCollum's proposal, but Mitchell prefers new letters, namely, F and G.

It does not seem likely that unanimity will b[e] reached unless the two sides will give and take, and so serve the interests of the cause. Therefore, we suggest a compromise here, namely, to designate th[e] antineuritic, less heat-stable vitamin, which Funk was the first to examine chemically and for which McCollum and Davis have chosen the letter B, by F(B), and the vitamin stimulating appetite and growth by G(B). The advantage of this nomenclature is that

[1] SCIENCE, 68: 206. August 31, 1928.

meets everybody's wishes and is not entirely itrary.

t would be better to retain the letter B for the sent, till those who are not workers in this field research have become used to the single F and G, the letter B stands for a certain conception which, enable though it may have proved to be, will but wly disappear from popular literature to make ce for a more correct term. The history of anti-hitic vitamin is there to remind us that new con-tions take time to penetrate, even now and then finds it identified with the vitamin A, and that circles where one would not expect this. It is to feared that the uninitiated, when suddenly con-nted with the letters F and G, will not know to at they refer. Of course the British proposers, o only want to prevent confusion, will not mind ether this aim is reached by means of letters or ures. Figures, with their quantitative character, uld seem to be less desirable than letters to distin-ish qualitatively different matter. It would be quite ssible for any one not to know, for the moment, ether 1 or 2 referred to the antineuritic vitamin, doubt which will not be so likely to arise concerning ters, especially not if, as in this case, they offer a emo-technical advantage, F being the initial of e name of the man who first tried (and at what ins!) to detect the chemical nature of the anti-uritic vitamin, while G reminds of Goldberger, who und the P(ellagra) P(reventive) vitamin, which ere is reason to suppose is identical with the more at-stable, growth-promoting component. If Funk uld have made up his mind to agree to the designa-on of the antirachitic vitamin by the letter D, as e majority of workers do, instead of by the letter E, e latter having already been taken by Evans to dicate the anti-sterility vitamin, a provisional agree-ent would have been reached and in the realm of the tamins it would be *tout pour le mieux dans le eilleur des mondes.*

E. C. van Leersum

Netherland Institute of Nutrition
Amsterdam

ULTRA-VIOLET EXHIBITS

I note in Science for December 7, 1928, a very eadable account of an operating ultra-violet exhibit the Natural Science Museum in London.

I am wondering if a great number of readers of cience might not get the idea from the article that ch a demonstration as there depicted is new, or, even ore, that it is the first time such a demonstration as been arranged in a museum. I believe it is quite enerally known that such exhibits in ultra-violet have een shown in laboratories, lectures and the like in this country hundreds of times, and that many thousands of people have witnessed them. I particularly wish to call attention to the fact that this museum in New York has an ultra-violet exhibit whereby the visitor may see the fluorescence and phosphorescence of a variety of materials by pressing a button and thereby illuminating the stage with ultra-violet.

The Museums of the Peaceful Arts has now had this visitor-operated exhibit working for a period of about nine months. The museum makes no claim that there is any new science presented by this exhibit, but it is found to be very interesting to all classes of visitors and is typical of a large number of exhibits in the institution. It shows not only the different fluorescent effects of various materials but also the effect upon a statue from the Bureau of Standards, revealing it in dress under ordinary illumination and with its dress disappearing under ultra-violet light.

I might mention that the Buffalo Museum of Science has recently set up a similar very interesting exhibit, possibly antedating the one described in the London *Times*. Also the Newark Museum of Science and the Philadelphia Museum are planning similar educational exhibits.

F. C. Brown

SPECIAL ARTICLES

A PRELIMINARY REPORT UPON THE UTILIZATION OF THE SPECTROPHOTOMETER IN THE DETERMINATION OF MINUTE AMOUNTS OF ALUMINUM[1]

The ready employment of spectrophotometric data for the successful quantitative determination of dyestuffs used as food colors[2] as well as for the micro-analytical determination of boron[3] has suggested the employment of the spectrophotometer as an aid for the accurate determination of aluminum.

The introduction of aurine tri-carboxylic acid as a qualitative test for aluminum by Hammett and Sottery[4] and its progressive employment as a quantitative reagent by Yoe and Hill[5] have led to its adoption by physiological and agricultural chemists.[6] The test consists in the formation of a red compound or absorp-

[1] Contribution from the Utensil Fellowship, Mellon Institute of Industrial Research, University of Pittsburgh.

[2] Mathewson, *Jour. Assoc. Off. Agric. Chem.*, 2: 164 (1916). Bureau of Standards Technical Paper No. 440.

[3] Holmes, *Jour. Assoc. Off. Agric. Chem.*, 10: 522 (1927).

[4] Hammett and Sottery, *Jour. Amer. Chem. Soc.*, 47: 142 (1925).

[5] Yoe and Hill, *Jour. Amer. Chem. Soc.*, 49: 2395 (1927).

[6] Myers et al, *Jour. Biol. Chem.*, 58: 598 (1928).

tion complex of aluminum with the aurine tri-carboxylic acid in an acetic acid solution. The excess of aurine tri-carboxylic acid is then decolorized by addition of an ammonia solution containing ammonium carbonate. In the absence of aluminum, a pale yellow color is observed at the completion of the reaction; in the presence of the metal, a residual red color is obtained and the intensity of the color is proportional to the aluminum content of the solution. Up to the present time this color has been compared with standards in the colorimeter or by using Nessler tubes.

The premise upon which the spectrophotometer was employed was that the spectral transmissive properties of the colored aluminum solutions should yield not only a more accurate measurement of the metal content as represented by the color intensity, but also a possibility of extension of the range of accurate quantitative estimation beyond the now feasible limit.

The present brief note summarizes our initial investigation during the latter part of December, 1927. Our purpose in publishing at this time is to announce our use of the spectrophotometer method in this field. A complete report will be published in the near future.

The dye, aurine tri-carboxylic acid, known also as "aluminon," shows considerable variation in properties when purchased upon the open market. Even samples of dye obtained at different times from the same manufacturer showed marked fluctuation in reagent quality. This non-uniformity of properties is probably due to presence of isomers and secondary reaction products, and necessitates careful standardization for each batch of dye employed.

Since the early investigations of Yoe and Hill had demonstrated the importance of the control of the hydrogen ion concentration, the initial transmission studies were made upon solutions of the dye, for the dual purpose of gauging its color change under varying hydrogen ion environment and the shift in the absorption maxima when aluminum was present. Accordingly, solutions were prepared from sulfuric and acetic acids, a requisite amount of aurine tri-carboxylic acid added to give a good final absorpti color (about a milligram per cc) and ammonia co taining ammonium carbonate added in sufficient qu tity to give the desired hydrogen ion concentrati After addition of water to a definite volume, t solutions were allowed to stand for twenty minutes t clear them of bubbles. Measurement of the transmi sion of a 2-cm layer of the solution was complet by a Hilger wave-length spectrometer, equipped wi a Nutting photometer upon which the extinction coe cient could be read directly. One slope of the absorp tion band was found to extend into the blue-gree region (480 to 580 mμ, showing a maximum at 525 m

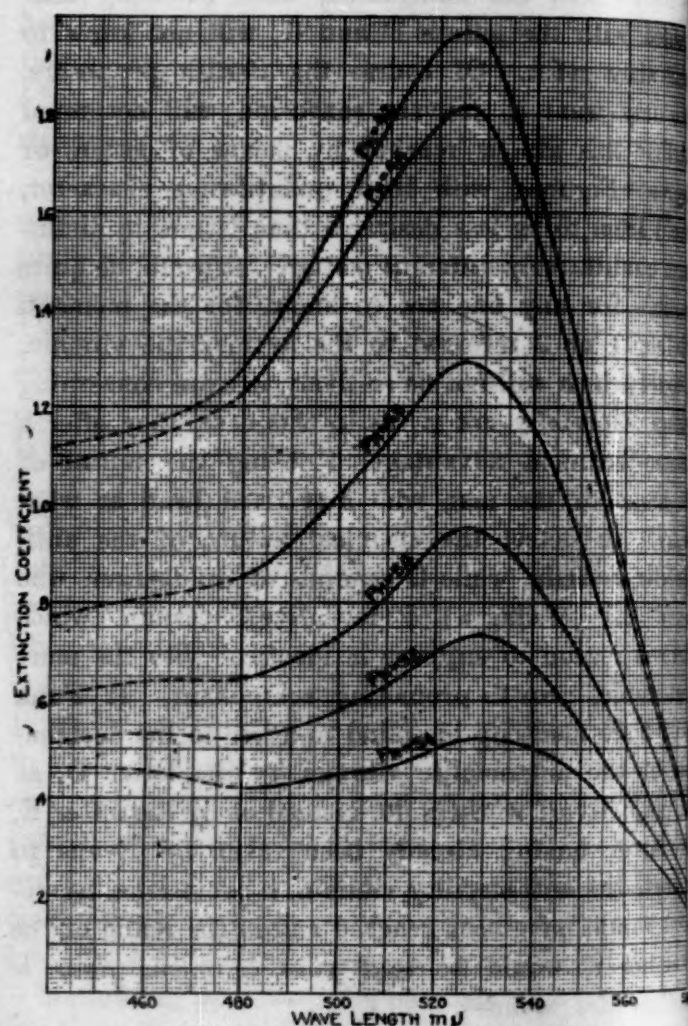

FIG 1 CURVES FOR AURINE TRICARBOXYLIC ACID AT VARIOUS HYDROGEN ION LEVELS

TABLE I

EXTINCTION COEFFICIENTS OF AURINE TRI-CARBOXYLIC ACID SOLUTIONS AT VARIOUS HYDROGEN ION LEVELS

pH	Extinction Coefficients										
	480 mμ	490 mμ	500 mμ	510 mμ	520 mμ	525 mμ	530 mμ	540 mμ	550 mμ	560 mμ	570 mμ	580 mμ
4.2	1.28	1.43	1.61	1.79	1.94	1.97	1.95	1.67	1.28	.85	.55	.36
4.4	1.22	1.36	1.50	1.66	1.78	1.82	1.79	1.56	1.19	.82	.53	.35
4.8	.85	.92	1.03	1.13	1.26	1.29	1.28	1.17	.92	.63	.44	.29
5.0	.64	.68	.73	.81	.92	.95	.94	.85	.69	.51	.33	.22
5.2	.52	.54	.58	.63	.70	.73	.73	.68	.55	.41	.29	.18
5.4	.42	.43	.45	.46	.50	.52	.52	.50	.45	.34	.25	.17

[illegible] this allowed evaluation of the extinction coeffi[illegible]s with considerable precision. The average value [illegible]ree readings was obtained, and the results are [illegible]ated below in Table I and plotted in Fig. 1.

[illegible]pon shifting attention to the solutions containing [illegible]inum, it was at once ascertained that the residual [illegible] due to aluminum complex after decolorization

The close relationship between the increase in the extinction coefficient with increase in aluminum content becomes more apparent near the maxima of the curve, and in the following table this increase has been shown at values of E for 540, 545 and 550 mμ by subtracting the value of E for the aluminum-free solution at these wave-lengths.

TABLE II

[illegible]NCTION COEFFICIENTS OF SOLUTIONS OF ALUMINUM-AURINE TRI-CARBOXYLIC ACID COMPLEX ADJUSTED TO THE SAME HYDROGEN ION LEVEL (pH = 7.0)

[illegible]entration of aluminum in [illegible]lligrams per 100 cc sol'n.	Extinction Coefficients								
	500 mμ	510 mμ	520 mμ	530 mμ	540 mμ	545 mμ	550 mμ	560 mμ	570 mμ
.03	1.41	1.56	1.75	1.99	2.15	2.17	2.10	1.76	1.32
.02	1.10	1.19	1.33	1.50	1.59	1.62	1.59	1.38	1.07
.01	.85	.89	.97	1.06	1.13	1.15	1.12	.99	.79
.005	.68	.72	.78	.84	.89	.90	.87	.78	.64
.0025	.61	.62	.67	.72	.73	.74	.72	.64	.54
None	.50	.51	.52	.56	.58	.59	.58	.56	.51

[illegible]he excess of dye was so weak that it was impos[illegible] to obtain satisfactory measurements in a 2-cm [illegible] Accordingly a modified Baly cell was devised [illegible]h allowed use of an absorbing solution about 20 [illegible] in length. This change allowed considerable [illegible]ction in the dye concentration of the test, and [illegible]surements were obtained which indicated that an [illegible]ease in the amount of added aluminum gave an [illegible]ost proportionate increase in the value of the [illegible]nction coefficient. It is interesting to note that [illegible] aluminum complex caused a shift in the maximum [illegible]he dye curve (changing it to 545 mμ) and giving [illegible]dder-colored solution. In the following table and [illegible]ph the values obtained for concentrations of [illegible]minum varying from twenty-five ten-thousandths [illegible] three one-hundredths of a milligram of aluminum [illegible] one hundred cc of solution are recorded.

TABLE III

[illegible]CREASE IN EXTINCTION COEFFICIENT WITH INCREASE IN ALUMINUM CONTENT IN AURINE TRI-CARBOXYLIC ACID SOLUTIONS

[illegible]minum concentration		Extinction coefficients			
[illegible]gm in [illegible]00 cc	parts per million	540 mμ	545 mμ	550 mμ	Average increase
.0025	1 in 40,000,000	.15	.15	.14	.15
.005	1 in 20,000,000	.31	.30	.29	.30
.01	1 in 10,000,000	.55	.54	.54	.54
.02	1 in 5,000,000	1.01	1.02	1.01	1.01
.03	1 in 3,300,000	1.57	1.56	1.55	1.55

For a further confirmation of the validity of the method even smaller quantities of aluminum were used. Measurements in this case were made with a Koenig-Martens spectrophotometer reading the angu-

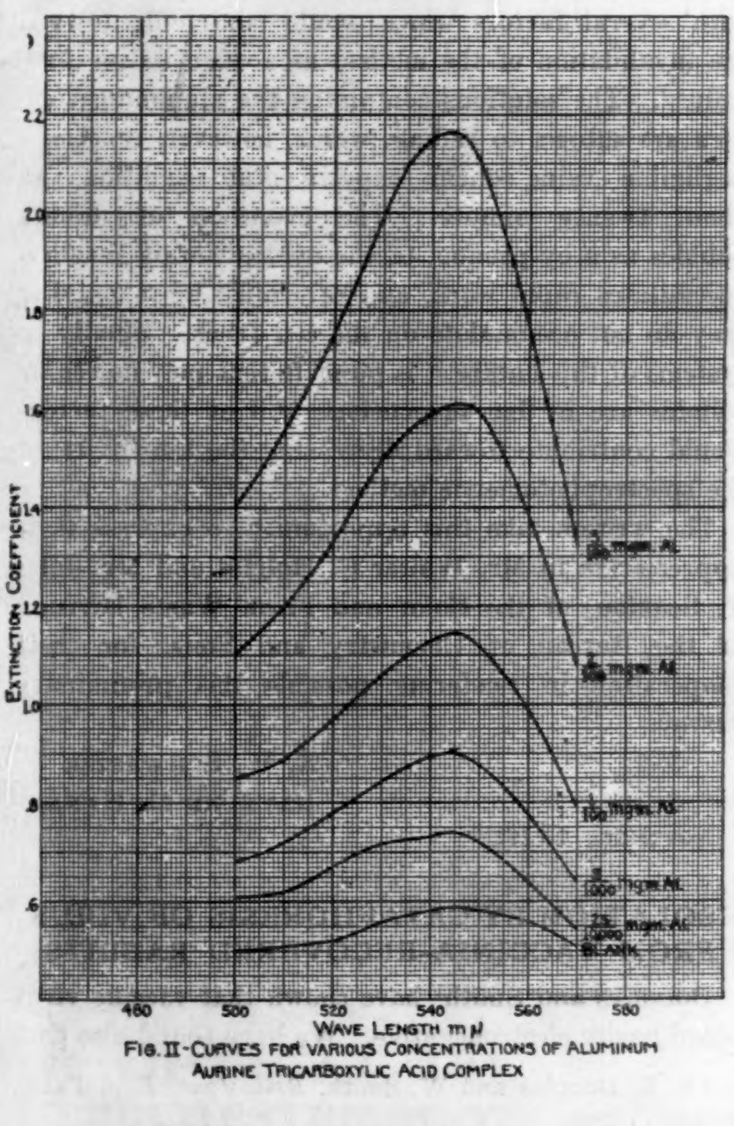

FIG. II—CURVES FOR VARIOUS CONCENTRATIONS OF ALUMINUM AURINE TRICARBOXYLIC ACID COMPLEX

TABLE IV

EXTINCTION COEFFICIENTS OF VARIOUS CONCENTRATIONS OF ALUMINUM AURINE TRI-CARBOXYLIC ACID COMPL
(ADJUSTED TO pH = 6.8)

Concentration of aluminum		Extinction coefficients					
mgm in 100 cc	parts per million	520 mμ	530 mμ	540 mμ	545 mμ	550 mμ	Increase in E 545 mμ
None	None	.61	.64	.65	.66	.63	–
.001	1 in 100,000,000	.68	.71	.71	.73	.70	.07
.002	1 in 50,000,000	.74	.78	.79	.81	.78	.15
.004	1 in 25,000,000	.80	.87	.91	.94	.91	.28
.005	1 in 20,000,000	.95	1.05	1.08	1.11	1.07	.45
.0075	1 in 13,300,000	1.03	1.16	1.23	1.26	1.22	.60
.01	1 in 10,000,000	1.14	1.28	1.36	1.39	1.34	.73
.02	1 in 5,000,000	1.67	1.84	1.97	2.01	1.94	1.35

lar rotation directly. The solutions were adjusted to a pH of 6.8. This proceeding served to reduce materially the fading with only a slight increase in the extinction coefficient value for the control solutions containing no aluminum. The results are tabulated in Table IV.

While these results in the minute concentrations do not show as sharp a proportionality as those previously mentioned, it may be stated in discussing the preliminary work here reported that we were troubled with several factors which would contribute toward the invalidation of the method as a quantitative procedure. The solutions containing the aluminum complex are subject to fading, which, however, reaches a negligible value under experimental conditions in about twenty minutes. The evolution of minute bubbles as a by-product of the neutralization reaction necessitates rapid work, and care must be taken that they do not accumulate on the cell faces. Finally in systems of this nature it is possible that polymolecular colloids are in fine suspension. All of these factors would contribute toward decreasing the sensitivity of the spectrophotometric method.

The writers take this opportunity to express their appreciation to Mr. Walter C. Holmes and Mr. John T. Scanlon, of the Color and Farm Waste Division of the Bureau of Chemistry and Soils, for their cooperation in rendering possible this preliminary survey.

E. W. SCHWARTZE
RAYMOND M. HANN

NOVEMBER 1, 1928

ISOLATION BY CATAPHORESIS OF VIRUS FROM VACCINIA-RECOVERED RABBITS

DOUGLAS and Smith[1] have shown that vaccine virus is ordinarily electronegative. We have found also that material containing vaccine virus wanders in an el trical field to the positive pole.

Cataphoresis was applied to recover virus pres in small amounts in tissues. For, by this method, t virus can be concentrated at the anode from suspe sion of tissues which fail to reveal infectivity by t usual tests of animal inoculation.

The conditions of cataphoresis were as follow time, 3 hours; milliamperage, 2 to 4.8; drop in pote tial, 1 to 2 volts; pH of the suspension (100 cc) o the tissue, 6.9 to 7.8.

By this method we were able to isolate active va cine virus in suspensions of testicles from rabbi which had recovered from experimental cutaneous va cinia. This was demonstrated by characteristic lesi in the skin and testicles of healthy rabbits injecte with anodic material. The animals used for cata phoresis tests were injected intracutaneously wi neurovaccine virus from twelve to fifty-six days pri to the experiment, and at the time their testicles we removed for examination the animals were wholly re covered from the cutaneous vaccine lesions and we apparently normal.

The isolation of the virus from animals wholly re covered from infection and in a healthy conditio and the failure thus far to obtain immunity in the ca of filterable viruses unless living virus is used[2] shoul be considered in relation to the possibility that im munity in virus diseases is linked with the presen in the body of living virus. The ideas here express are being investigated at the present time.

PETER K. OLITSKY,
PERRIN H. LONG
ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH,
NEW YORK, N. Y.

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[2] *Cf.*, "Filterable Viruses," edited by T. M. Rivers, Baltimore, 1928; and P. K. Olitsky and P. H. Long, *Jour. Exper. Med.*, 47: 835. 1928.